PROCEEDINGS

OF THE

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CONTENTS.

VOL. LXIV.

No. 402.

No. 403.

No. 404.

No. 405.

No. 406.

No. 407.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

"Report on the Expedition to Sahdol, Rewah State, Central India, to observe the Total Solar Eclipse of 1898, January 22." By W. H. M. CHRISTIE, C.B., M.A., F.R.S., Astronomer Royal, and Professor H. H. TURNER, M.A., F.R.S. Received May 25, 1898.

The Report is presented in three parts.

Part I is a joint Report by the two observers.

Part II is a separate Report by the Astronomer Royal; and

Part III is a separate Report by Professor Turner.

PART I.

1. This expedition was organised by the Joint Permanent Eclipse Committee of the Royal Society and Royal Astronomical Society, funds being provided from a grant made by the Government Grant Committee.

The Government of India made excellent arrangements for the party, and the Surveyor-General of India with the staff of his Department rendered great service in selecting a site, clearing the jungle, establishing a camp, erecting the instruments, and in giving every assistance in the observations, for all of which the observers desire to tender their thanks.

The observers are also indebted to the Great Indian Peninsular Railway, the Bengal and Nagpur Railway, and the East Indian Railway for great liberality in granting facilities and in making special arrangements for the safe conveyance of their instruments from Bombay to Sahdol.

2. *Personnel.*—The following persons took part in the expedition:—

W. H. M. Christie, M.A., F.R.S., Astronomer Royal.

H. H. Turner, M.A., F.R.S., Savilian Professor of Astronomy at Oxford.

Harold A. H. Christie, *who gave the exposures throughout the eclipse for the Astronomer* Royal.

It was originally proposed that Dr. A. A. Common, F.R.S., should also take part in this expedition, but he ultimately found that he was unable to do so, and Dr. Copeland, Astronomer Royal for Scotland, was invited by the J.P.E. Committee to go in his place. Dr. Copeland preferred, however, not to join any of the three other expeditions, but to establish himself independently.

3. *Itinerary.*—The observers left Marseilles in the Peninsular and Oriental steamship "Ballaarat" (R.M.S.), on Thursday morning, December 16, 1897, their instruments having been put on board this vessel in London ten days earlier. They arrived at Bombay on Monday morning, January 3, 1898, the weather during the voyage being excellent. After a few days spent in landing the instruments and arranging for their journey to Sahdol, they left Bombay on Friday evening, January 7, travelling direct to Sahdol by special arrangements courteously made by the G.I.P., East Indian, and Bengal and Nagpur Railways, and arrived at Sahdol in the early morning of Sunday, January 9.

4. *Selection of a Station.*—The J.P.E. Committee originally proposed for this expedition a station south of Poona, either near Karad on the S. Maratha Railway or near Jeur on the G.I.P. Railway, other expeditions occupying stations near Viziadrug on the coast, and Pulgaon on the Nagpur branch of the G.I.P. Railway.

The Surveyor-General of India having offered to give every assistance to the expeditions, appointed Major Burrard, R.E., to make all necessary arrangements, including the determination of exact local time and of the longitude and latitude of the station. Major Burrard selected Karad, in the Satara district, as the best station. Owing, however, to the outbreak of plague at that place, and its prevalence in the Bombay Presidency, it was finally decided, on the advice of the Bombay Government, to abandon this choice and to occupy a station at Sahdol, on the railway to the east of Pulgaon, connecting Katni and Bilaspur. As this site was in dense jungle, it was necessary to clear a considerable space for the camp, part of which was to be occupied by a party under Mr. Michie Smith, Government Astronomer at Madras. This clearing, the establishment of the camp, and the erection of piers and huts for the instruments and of a dark room for photography, were all admirably carried out by Major Burrard, R.E., and his assistant, Lieutenant Crosthwait, R.E., before the arrival of the observers, who thus found everything ready for the setting up of their instruments.

5. *Position of Station.*—The observing station was about three-quarters of a mile from the railway station, on the south side of the line. The position of the centre of the pier on which the cœlostat used by the Astronomer Royal was mounted is—

Longitude 81° 21′ 33″ E. = $5^h\ 25^m\ 26^s.2$ E.
Latitude 23° 16′ 45·3″.
Height above mean sea level 1502·4 feet.

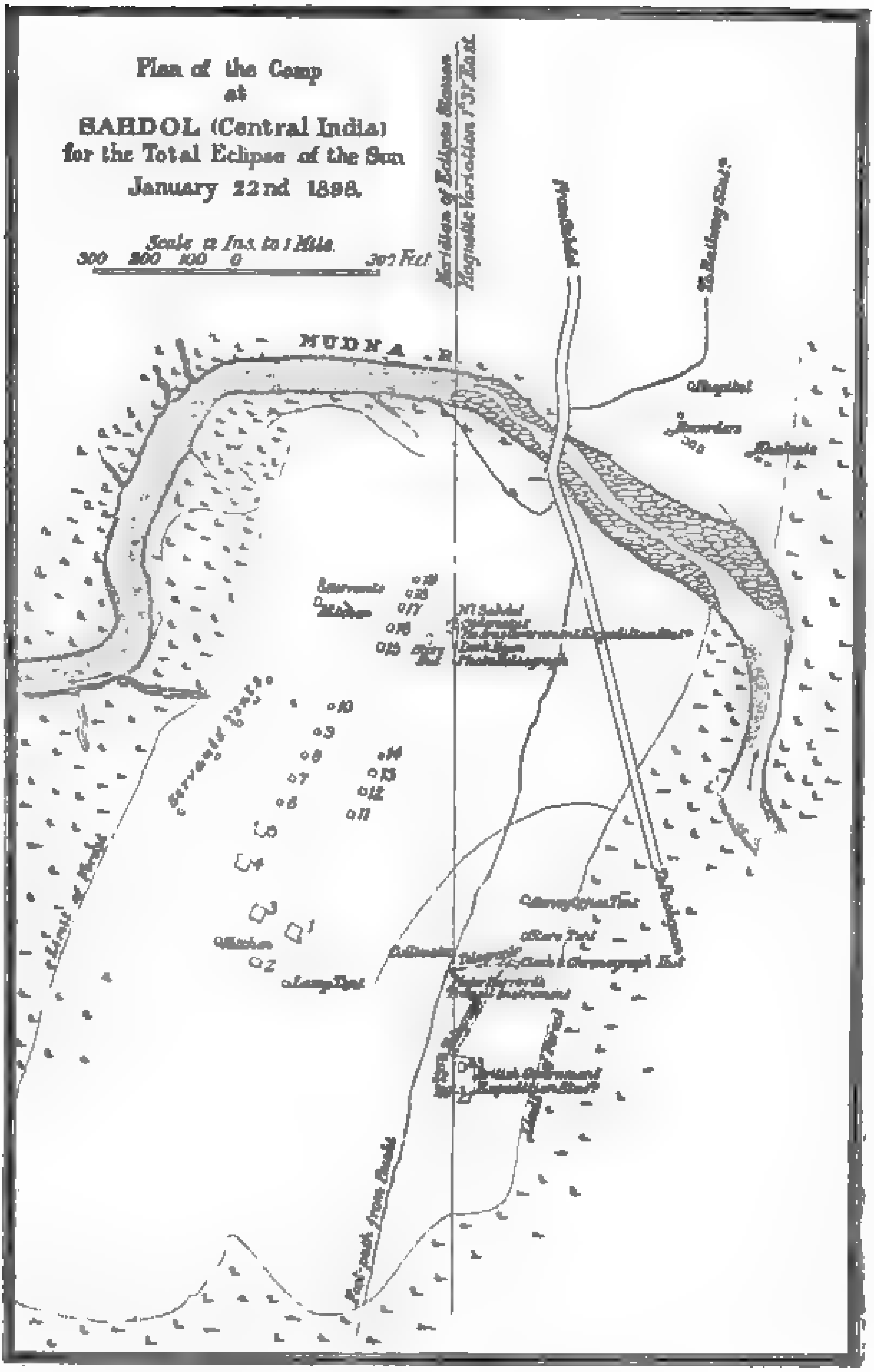
This position was determined after the eclipse by Major Burrard, R.E., by accurate triangulation, connecting the site with the principal triangulation of the Survey of India.

Professor Turner's cœlostat was 20 feet due east of this, and the transit instrument (used by Major Burrard in his time determinations) 240 feet due north of it.

This spot is 4 miles towards the south-east from the central line as shown in the 'Nautical Almanac' and 'American Ephemeris,' 3½ miles from the line as shown in the 'Connaissance des Temps,' and 2 miles from the line as shown in the 'Berliner Jahrbuch.' It may be remarked that the data for predicting the four contacts given in the 'Nautical Almanac Circular No. 16' were found insufficient, the nearest points for which approximate formulæ were given being in longitude 79° (Nagpur) at a considerable distance from the central line, and in longitude 83° (south of Benares), too far away to give accurate predictions. [See Professor Turner's separate Report.]

6. *The Camp.*—The general arrangements of the camp (see next page), which consisted of more than fifty tents and huts, were, as already mentioned, admirably carried out by Major Burrard, R.E., it being necessary to clear a considerable space (about 700 by 300 yards) in the jungle by burning and felling trees, in order to set up the numerous tents of the living camp at some distance from the observing huts. On part of this clearing the Government Astronomer at Madras (Mr. Michie Smith) and his assistants erected their camp and instruments, and Major Burrard rendered considerable assistance to this party. There were also several tents for occupation by distinguished officers of the Survey, and we had the pleasure of seeing at the camp on the day of the eclipse the Surveyor-General (General Strahan, R.E.), General Woodthorpe, R.E., Colonel Sir T. Holditch, R.E., and other officers, and for a few days preceding the eclipse Colonel Gore, R.E., who left us for the Pulgaon Camp on January 19.

7. *Meteorological Conditions.*—From valuable information collected by Mr. Eliot, Meteorological Reporter to the Government of India, it appeared that the chances of fine weather were practically the same all along the line of totality, there being very little risk of cloud, though some chance of dust interfering with the definition. The thickly wooded country round Sahdol seemed well adapted for protection from the danger of dust, and during the days near the eclipse the ground near the instruments was covered with straw and watered in the morning to prevent excessive heating of the air in the



REFERENCE.

1. { Mr. W. H. M. Christie, C.B., F.R.S.
   { Master Harold Christie.
2. Lieut. H. L. Crosthwait, R.E.
3. Mess.
4. Professor H. H. Turner, F.R.S.
5. Major S. G. Burrard, R.E.
6. Mr C. H. McA'Fee.
7. Mr. R. George.
8. *Captain E. D. Bullen, R.E.*
9. *Colonel Sir T. H. Holditch, K.C.I.E., C.B., R.E.*
10. *Captain J. A. Dealy, R.E.*
11. { *General C. Strahan, R.E.*
    { *Colonel St. G. C. Gore, R.E.*
12. *General R. G. Woodthorpe, C.B., R.E.*
13. *Mr. C. Michie Smith.*
14. Captain W. Ewbank, R.E.
15. { Mr. A. H. Campbell, I.C.S.
    { Surgeon-Major J. L. Van Geyzel, I.M.S.
16. Madras Mess.
17. Doctor J. W. Evans.
18. { Mr. P. W. Lawrence.
    { Mr. A. F. N. Moos.
19. { Mr. R. Ll. Jones.
    { Mr. H. Kelsall Slater.
20. Mr. Christie's coelostat { Lat. 23° 16′ 45″·3 N. Long. 81° 21′ 33″·0 E. Height 1562·4 feet.
21. Professor Turner's coelostat.
22. Dark room.
23. Instrument tent.

path of the rays from the sun to the cœlostats, which were placed at a height of only 2 feet 8 inches from the ground.

The character of the weather was practically uniform during our stay at Sahdol. At night the minimum temperature was about 32° up to January 20, afterwards a little warmer (not falling below 39°, on January 23 and 24). At about 7.30 A.M., soon after sunrise, the temperature began to rise rapidly, attaining a maximum of about 80° at about 2 P.M., and remaining near this point from noon till about 4.30 P.M., when a fall nearly as sudden as the rise began. The air was nearly saturated with moisture at night and very dry in the daytime. There was heavy hoar frost at night.

These conditions seriously limited the time available for photography, which could only be carried on conveniently during the hot part of the day, say from 10 A.M. to 6 or 7 P.M., as there was no ready means of warming the water in the dark room.

The first clouds seen since the party landed in India were light fleecy clouds in the evening of Wednesday, January 19, and more appeared in the early mornings of January 20 and January 21, dispersing in each case between 10 and 11 A.M. The day of the eclipse was perfectly cloudless throughout, and the definition good.

[For detailed readings of thermometers and barometer, see separate Report by Professor Turner.]

8. *Instruments, &c.* [See separate Reports of the observers.]

9. *Huts.*—The chances of rain being so small, waterproof huts were not considered necessary, and the instruments were protected by rush thatchings laid on a framework of bamboo, which could be readily removed as required.

10. *Assistance.*—The observers were assisted in the exposures as below:—

The Astronomer Royal—

Mr. McA'Fee............	Recorded times of exposure.
Mr. Harold Christie......	Exposed at objective.
Vishnoo Babaji Garnd....	Handed plates.
Venayek Narayan	Received plates.

Professor Turner—

Dondu Venayek.........	Exposed at objective.
Tukaran Hanmant.......	Handed plates.
Shankar Devidas	Received plates.

Also

Govind Balwant Joshi....	Counted seconds aloud.
Narayan Vishnoo Apte....	

A few seconds before totality, as shown by the diminishing crescent of the sun, Professor Turner was to call "Get ready"; at totality *to call sharply (the monosyllable* "Tup" was used). The first time-

keeper immediately started the stop-watch and proceeded to count aloud "one, two, three," &c., up to sixty, when the second timekeeper took up the counting, "one, two, three," &c. By having two timekeepers, opportunity was given to both to see the eclipse.

The operations were rehearsed on every day of the week preceding the eclipse, at the time of totality, and on two days also at dusk, with lamps.

A photographic hut with a supply of chemicals was supplied from the Calcutta office of the Surveyor-General's Department, and Mr. George, of that department, was told off to assist in the photographic work. He developed some of the photographs taken by the Astronomer Royal during the partial phases.

11. *The Day of the Eclipse.*—Perfectly clear throughout. The morning was spent in final preparations. The first contact occurred at 0 hr. 13·3 mins. local mean time, and the Astronomer Royal proceeded to take nine photographs of the partial phase, the first being exposed at 0 hr. 14·6 mins., and the last at 1 hr. 22·2 mins. Totality commenced at 1 hr. 41·1 mins., and lasted about 105 seconds, during which time the programmes detailed below were successfully carried out. Nine more partial phase photographs were taken between 1 hr. 59·4 mins. and 2 hrs. 59·2 mins. The fourth contact was at 3 hrs. 1·6 mins. (see accurate times below).

There was a good deal of light during totality, and lamps were not in any way needed. The temperature fell 4·5° between first and second contacts, and another 3·5° between second contact and 2 hrs. It had practically returned to its normal value by the fourth contact (see accurate readings below). But the fall of temperature did not nearly represent the sensation of chill. At 1.15, when the air felt distinctly chilly, the temperature had only fallen 2°.

There was no appreciable "shadow" effect at totality, nor was any such effect noticed by two observers (General Woodthorpe, R.E., and Colonel Sir T. Holditch, R.E.) from the top of a hill a few miles away, close to the central line. These observers did, however, notice the well known "shadow bands" on the table they had prepared for sketching, without having previously heard of these bands in any way.

We had not many opportunities of observing the behaviour of animals. Kites which had been circling round the camp flew off to the surrounding trees some minutes before totality, and about the same time we heard cries from the village of Sahdol. We were told by another observer (Professor E. G. Hill, of Allahabad) that at Buxar he had noticed a herd of goats get into line and march homewards; that two mongooses in a hole in a bank had seemed very much frightened; that squirrels were silent during totality, and that a kingfisher began catching fish.

PART II.—SEPARATE REPORT BY MR. W. H. M. CHRISTIE.

The programme of observation was composed of two distinct parts—(1) Photographs of the corona on a large scale during totality; (2) photographs of the partial phase before and after totality for determination of the position of the moon relatively to the sun.

The instrument used in both cases was the photographic telescope by Grubb, with object-glass of 9 inches aperture and 8 ft. 6 in. focal length (presented to the Royal Observatory by Sir Henry Thompson), to which a concave compound lens by Dallmeyer, of 3 inches diameter and 12 inches focus, had been fitted as a secondary magnifier, placed a short distance within the focus. This combination gave an image of the sun 4 inches in diameter, and a field (for full pencils) of nearly 10 inches diameter, so that the corona to a distance of one and a half radii from the Sun's limb would be included in the field. A cœlostat, specially designed by Dr. Common, carrying a plane silver-on-glass mirror of 16 inches diameter, made by him, was employed to reflect the rays into the Thompson coronagraph, which was firmly mounted on two brick piers, so as to point to the mirror at an angle of depression of about 10°, and to be placed in an azimuth of about 17° north of west for the day of the eclipse. The camera was furnished with eight plateholders, taking 12 × 10 in. plates, seven being reserved for use during totality, and the eighth fitted with a Thornton-Pickard instantaneous focal-plane shutter, to take photographs on 8½ × 6½ in. plates during the partial phases, for determination of the moon's position.

The seven slides for photographs of the corona during totality were exposed as below, the exposures being given with a screen held

No.	Exposure.			Plate.	Developer.
	Beginning.	End.	Duration.		
	Secs.	Secs.	Secs.		
1	6	7	1	Ilford ordinary..........	Hydroquinone dilute.
2	12	17	5	Ilford rapid..............	,, ,,
3	24	34	10	,, ,, (backed)......	,, ,,
4	40	60	20	Rocket (backed)..........	,, ,,
5	67	75	8	,, ...	Eikonogen.
6	81	82	½	Ilford ordinary	Hydroquinone dilute.
7	89	91	2	*Gazelle* (dry collodion)....	,, ,,

in front of the object-glass by my son Harold, and the times, reckoned from the commencement of totality (2nd contact), being recorded by Mr. McA'Fee, of the Indian Survey Department.

It had been intended to use Hill-Norris dry collodion "Gazelle" plates for three of the slides (the fineness of grain as compared with gelatine plates giving them a marked advantage), but from trials made before the eclipse it was found that these plates were for some reason untrustworthy. I, however, thought it well to expose one of these plates, but the result is not satisfactory.

The sky was cloudless during the eclipse, and the programme was carried out without a hitch, with the aid of two native assistants of the Survey Department (Mr. V. B. Garnd and Mr. V. Narayen), who respectively handed me the slides and received them from me, and there was fifteen seconds to spare before the end of totality, the duration at Sahdol being 1 min. $45\frac{1}{2}$ secs. as observed.

For the partial phase, nine photographs were taken between first and second contacts, and eight between third and fourth contacts, as well as a photograph for orientation (with double exposure) immediately before and after the eclipse. The aperture of the object-glass was reduced to 3 inches for these photographs, as it was found by trials before the eclipse that with the aperture thus reduced the exposure given by the Thornton-Pickard shutter set to its highest speed was satisfactory for the slow plates used (Thomas's lantern plates). The times of exposure were recorded on a chronograph, a key being pressed by me in the left hand at the same instant as the exposure was given by pressing a pneumatic ball in the right hand. The times of the fall of the shutter were also independently recorded by Mr. McA'Fee with a chronometer carefully compared with the transit-clock. All the arrangements for accurate local time, which was of vital importance for this part of the programme, and for determination of the longitude of the station by connection with the principal triangulation of the Survey of India, were most ably carried out by Major Burrard, R.E., and Lieut. Crosthwait, R.E. The position of my instrument, as found by them after the eclipse, was Long. $81^\circ 21' 33'' = 5^h 25^m 26^s{\cdot}2$ E., Lat. $23^\circ 16' 45{\cdot}3''$ N. Altitude above sea-level 1502·4 feet.

The coronagraph was carefully focussed before the eclipse by use of the method described in the Report of the Eclipse Expedition to Japan, 1896,* the image of an object (gauze net in the plane of the plate), being photographed by reflection normally from the plane mirror of the cœlostat. A special spare back for the plateholder was prepared with a hole covered with gauze net just above the centre, and one of the 12×10 in. plates being cut in two, one half was placed *in the lower half of* the plateholder and the reflected image photo-

* *'Monthly Notices,'* R.A.S., vol. 57, p. 105.

graphed on it at different parts of the field, the source of light being a paraffin lantern. The adjustment to focus was made by moving the secondary magnifier, rings of paper placed between flanges on the adapter carrying the magnifier and its mounting giving the means of doing this with great nicety. By this method, in which the error from imperfect focus is doubled by the double passage of the rays through the telescope, it was found that a displacement of the magnifier through the thickness of a sheet of paper (0·005 inch), representing 1/20,000th part of the focal length, made a sensible difference. The focus was thus obtained with great accuracy, as is evidenced by the sharpness of the image on the photographs taken during the partial phase.

The photographs with exposures of 8 secs., 10 secs., and 20 secs. all show coronal structure up to the edge of the plate, the streamer in the S.W. being particularly bright. A hurried eye estimation made by me during the 20 seconds' exposure gave the extreme extension of this streamer as about two-thirds of the distance of Venus from the Sun's centre, or about $3\frac{1}{2}°$ from the limb. The field being necessarily limited by the diameter of the concave enlarging lens (3 in.), it would be desirable to have one made of larger diameter for future eclipses so as to allow of the use of larger plates.

A comparison of the whole series of photographs indicates a close correspondence between the coronal streamers and the prominences visible, this correspondence being particularly striking in the case of three prominences in the N.W. quadrant from which three coronal rays rise, arching over at a distance of about 7′ from the limb and uniting to form one component of the striking long double ray in that quadrant. Other prominences on the Sun's limb appear also to be associated with extensions of the corona. The form of the corona bore a closer general resemblance to those of 1886 and 1896 as photographed than would have been expected considering that the date of the eclipse was so much nearer the epoch of minimum sun-spots, and in this connection it may be noted that there were three important groups of spots on the Sun about the time of the eclipse, and that a series of magnetic disturbances of moderate amount were recorded at Greenwich just prior to the eclipse, from January 15 to 21 continuously, indicating an unusual state of magnetic activity in close correspondence with the solar activity as evidenced by the Sun spots.

Part III.—Separate Report by Professor Turner.

Instrumental Equipment.

1. *The Camera.*—The double camera used in the Fundium Expedition of 1893 by Sergeant Kearney, and taken out to Japan in 1896 without result. The tube is of wood, 6 feet long and 14 × 7 inches in section, divided by a partition into two tubes of 7 × 7 inches section. In one of these is placed the "Abney"* lens of 4-inch aperture and 62-inch focal length, giving an image of the sun 0·57 inch in diameter; in the other the photoheliograph objective No. 2 (used in *Transit of Venus* expeditions), of 4-inch aperture and 5-feet focal length, with a Dallmeyer secondary magnifier of 7½ inches focus placed 5 inches within the focus, and giving an image of the sun 1½ inches in diameter; the camera furnished with six plate-holders, each taking two plates of 160 × 160 mm. (as in use for the Astrographic Chart), both plates being exposed by a quarter-turn of one shutter.

2. *The Cœlostat.*—The camera was pointed to a 16-inch cœlostat, the mirror of which was made by Dr. Common, the mounting and clock by Mr. J. Hammersley from designs by Dr. Common.

3. *The Polariscope.*—On the tube, and pointing to the same cœlostat, was a polariscopic apparatus consisting of an ordinary slit spectroscope (and telescope), with an Iceland spar double image rhomb substituted for the ordinary prisms. The instrument in its normal state had been used by Mr. Maunder in the 1886 eclipse expedition for photographing the spectrum of the corona. It is fitted with two plate-holders. The dimensions are as follow :—

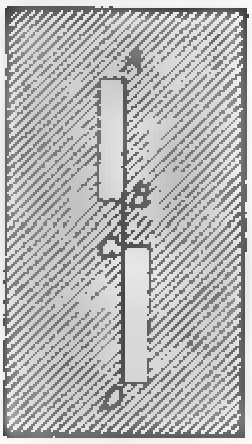
Objective......	3½	inches aperture,	18	inches focal length.
Collimator.....	1½	,, ,,	6½	,, ,,
Camera........	2	,, ,,	9	,, ,,

The use of the apparatus will be understood from the analogy of the ordinary spectroscope, regarding the Iceland spar rhomb as a prism giving only two colours—the two kinds of polarised light. We can thus use a wide slit, opening it until the two images of the slit are just in contact without overlap. This was found to happen with the apparatus in use with a slit of width 0·19 inch. The

* One-half of a doublet photographic lens by Dallmeyer, belonging to Captain Abney, used in the Eclipse Expeditions of 1886, 1887, 1889, 1893, and 1896, acquired from him by the Royal Astronomical Society in 1893 for permanent use in eclipse expeditions, in consideration of their replacement by two others.

diameter of the sun's image thrown on the slit being 0·21 inch, it was thought best to take the two photographs, so as to include opposite limbs of the eclipsed sun, and as much of the corona as could be got. The change from one portion of the sun to another was arranged by having two slits as follows:—

The two slits were cut in a piece of blackened card, the actual size of which is shown in the diagram. If the line ABCD had been diametral to the sun's or moon's image, half the moon and corona would in the first instance have appeared in the middle of the lower slit, CD. In this position the first exposure was given, producing on the plate two images side by side of the right limb of the eclipsed sun, one image being due to light polarised in a plane



parallel to CD, and the other to light polarised in the perpendicular plane. For the next exposure the card was slipped down in the groove in which it fitted lightly, and the left limb of the eclipsed sun fell on the middle of the slit AB; and so two images of the rest of the corona would be obtained. By combining the two photographs, we could get a double picture of a slice of the corona 1·0 inch long and 0·40 inch wide, containing the moon centrally; or, translating inches into diameters of the moon's image, five diameters long and two diameters wide. As a matter of fact, the image of the sun was not adjusted centrally on the line ABCD, and a good deal more of the moon's disc appeared on one photograph than on the other, this being deliberately arranged for a special reason, which will appear in discussing the photographs obtained.

Instrumental Adjustments.

5. *Adjustment of Cœlostat.*—The adjustment of the polar axis was made as described in the Report on the Japan Expedition* by means of the attached declination theodolites.

The following are the actual observations, those with the level

* '*Monthly Notices*,' vol. 57, p. 102.

being made on the meridian, and compared with the known latitude so as to give the same sign to the errors as the sun observations:—

Date.	Sun's H.A.	Obs. decl.	Tab. decl.	O.—C.
Jan. 11....	−4·0	−21° 45′	−21° 50′	+5′
		Observation with level		+3
	(Adjusted in azimuth and altitude.)			
	−3·0	−21 50	−21 49	−1
		Observation with level		0
	−1·0	−21 49	−21 48	−1
Jan. 17....	−3·7	−20 47	−20 45	−2
		Observation with level		0

[These observations were made with a dummy axis, before the mirror and its cell were mounted on January 19.]

Jan. 21....	−3·0	Observation with level		+6

[Adjusted level: probably disturbed by the weight of the mirror and its cell.]

	−2·0	Observation with level		0
	−2·0	−19 54	−19 54	0

[As there seemed some vibration in the instrument, the end of the arc holding the mirror was supported by a wooden block driven tightly under it; this again altered the level, which was readjusted.]

	0·0	−19 51	−19 53	+2
	0·0	Observation with level		+2
	+2·0	−19 49	−19 52	+3
Jan. 22....	−4·5	−19 37	−19 42	+5
		Observation with level		+2
„ 23....	−1·0	−19 24	−19 26	+2
		Observation with level		+3

It will be seen that the introduction of the wooden block as a partial support to the cœlostat disturbed the adjustments slightly, but it did not seem advisable to attempt to correct these small errors, which would have been a troublesome process with the stress of the wooden block to consider. The block certainly deadened the vibrations.

6. *Tilt of Mirror.*—If the mirror is not parallel to the axis of rotation the image will not be quite stationary.* Both instruments were tested for this error by reversing the mirror and cell in the Y's and noting the consequent displacement of the sun's image on the ground glass in the focal plane of the telescopes. The displacement,

* 'Monthly Notices,' vol. 56, p. 417.

giving double the inclination of mirror to axis, was found by C. on January 19 to be 7·2′; and was corrected after several trials to be 0·8′, denoting an error of 0·4′. The correction was made by screwing a short screw, made for the purpose, into one of the holes at the back of the mirror cell. [There are three of these holes, into which long screws are screwed in the operation of inserting the mirror into its cell.] T.'s mirror was examined on the same day, and being found to be sensibly parallel to the axis, no correction was necessary.

7. *Focussing of Telescopes.*—The method adopted was that described in the Report on the Japan Expedition.* The telescope was pointed normally to the mirror, and the image of a bright point or object in the plane of the film was photographed on the film.

A glass plate being cut in two, one half was blackened with asphalte, and the word "astronomy" printed on it by scratching with a needle point. This was the object photographed in the following experiments. Gauges had been made in England by means of which the three lenses (the Abney lens, the photoheliograph lens, and the Dallmeyer enlarging lens) could be set very accurately to positions in the wooden tube giving good focus. Positions at definite distances from these could be obtained by unscrewing the object glasses through fractions of a turn. If positions were required *within* the gauge position, the object glass was first *pushed* in slightly; and then it was noted what fraction of a turn it must be unscrewed to bring it to the gauge-position. Designating the gauge-position by zero, and one turn out by +1·0, one turn inside by −1·0, the following positions were tried:—

Abney Lens.

Plate 1, January 16, 0·0, +0·5, +1·0 positions. Three images on same plate, 0·0 the best.

Plates 2—5 were devoted to experiments on illuminants. A candle was found most convenient after all.

Plate 6, January 16, 0·0, −1·0, −1·5 (+0·5) positions; (+0·5) not on plate? 0·0 the best, if exposures rightly identified.

Plate 7, January 16, −1·5, −0·5, +0·5 positions; −0·5 the best of these.

Plate 8 (January 16). To make sure pushed still further in.

The above differences were slight, −3·0, −2·0, −1·0 positions, −1·0 the best.

Hence on this day focus seems at −0·5, or very close to it.

* 'Monthly Notices,' vol. 57, p. 105.

Dallmeyer Lens and Enlarger.

Plate 9, January 16, −1·0, 0·0, +1·0, +2·0 positions; 0·0 the best.

Plate 10, January 16, −0·5, +0·5, +1·5 positions; +0·5 the best.

Plate 11, January 17, 0·0, +0·25, +0·5, +0·75, 0·0 positions. Both 0·0 exposures were worse than the others.

Of the others 0·25 was perhaps best near the centre, and 0·5 (or 0·75?) further from centre.

Hence 0·5 was adopted, *i.e.*, the object glass was screwed one-half turn out. The screw has 12 turns to the inch; and the focussing was thus correct to 0·02 inch, as far as could be judged.

Abney Lens—(continued).

Plate 12, January 17, −1·0, −0·5, 0·0, +0·5, +1·0 positions; −1·0 best; −0·5 very good; 0·0, +0·5, +1·0 distinctly inferior.

This contradicts nothing but plate 6, on which the exposures may be wrongly identified, and if on that plate 0·0 is missing instead of +0·5, then −1·0 would be the best on the plate. *Hence* −1·0 *was adopted.*

[At 25 turns to the inch this focus was also correct to 0·02 inch.]

8. *Programme of Observations.*—The six slides for photographs of the corona were filled as below, the same plates being used for the Dallmeyer and Abney lenses in each case, and standard squares having been impressed on plates 2, 4, 5, 6, by Captain Hills, R.E., before sending the plates out from England.

No. of slide.	Exposure.	Plate.
1	1 sec.	Dry collodion "Gazelle."
2	5 secs.	Ilford "Empress."
3	10 "	Rocket.
4	20 "	Ilford "Rapid."
5	1 sec.	Ilford "Empress."
6	2 secs.	Ilford "Empress."

Besides these the two exposures through the polariscopic apparatus were made, each of 5 seconds' duration, Paget plates being used.

The developer used was amidol for all these plates, which were all successfully exposed.

9. *Times of Contacts.*—These were independently observed by Professor Turner and by Major Burrard, R.E. The former used for first and fourth contacts a pocket watch which was compared with the sidereal clock at 8.5 A.M. and found 29 seconds fast; and again at 8 P.M., and found 26·9 seconds fast, of local mean time. At second and third contacts he gave a sharp signal, the time of which was

noted by Mr. McA'Fee on a mean time chronometer, carefully compared with the clock both before and after. The contacts were observed by throwing a 4-inch image of the sun on white paper from a navy-pattern telescope. [Aperture 2 inches, focal length 28 inches, magnifying power of negative eyepiece 25.]

Major Burrard observed with a 2-inch telescope, and recorded his times on the chronograph in connection with the sidereal clock.

This clock was in a grass hut and subjected to a daily variation of 50° of temperature, but great care was taken with the star observations, as will be explained in detail in the final report of the Astronomer Royal. The local mean times of the star observations for clock error (by Major Burrard) were

January 21 days 18 hrs. 30 mins. 58 secs., and January 22 days 7 hrs. 21 mins. 13 secs, and the errors of the clock at these times were, +0 min. 12·83 secs. and +0 min. 14·88 secs. respectively.

Local Mean Times of the Four Contacts.

Observer H.T.	Observer B.	Calculated time.	Calculated from 'N.A. Circular, No. 16,' with formulæ for	
			Nagpur.	Benares.
hrs. mins. secs.	secs.	secs.	secs.	secs.
I. 0 13 11·7	22·8	6·3	11·3	8·8
II. 1 41 3·0	3·3	5·4	12·3	9·3
III. 1 42 48·5	47·9	52·0	67·3	49·3
IV. 3 1 40·2	35·7	51·3	63·7	45·3

10. *Remarks on Formulæ for Prediction of Contacts.*—As was mentioned in paragraph 5 of the joint report of the Astronomer Royal and Professor Turner, the data for predicting the four contacts were found insufficient. This will be seen by comparison of the last two columns, wherein the times as given by the published formulæ most nearly suitable for Sahdol are given. On application to the Superintendent of the 'Nautical Almanac,' he kindly supplied the data for the third column, giving the G.M.T.s of the four contacts, to which the longitude of Sahdol has been applied.

[The position of Sahdol is

5 hrs. 25 mins. 26·2 secs. E., Lat. 23° 16′ 45″ N.]

As his letter contains a suggestion which may be useful to other observers, I reproduce it here.

"Nautical Almanac Office,
"March 26, 1898.

"Dear Turner,

"You will find the deduction of the formulæ (used in 'N.A. Circular No. 16' and others) in appendix to N.A. 1836, p. 117. These equations will give sensible accuracy for a distance of about 50 miles from the point for which they are computed. In this case the distance is about 150 miles.

"I find that the most expeditious way of getting accurate results in this work, is to use the Besselian Elements (page 4 of circular No. 16). You can infer the time of middle of eclipse to the nearest minute from the Table on p. 3 of circular, and then 10 minutes work gives you the times of beginning and ending of totality with sensible accuracy.

"Of course for accurate determinations of times of first and last contacts (partial phase) special computations for the approximate times of these contacts would be necessary.

"Yours very truly,
"A. M. W. Downing."

The following remarks on the geometrical significance of these formulæ may not be out of place.

The time of a contact t at a place of geocentric latitude l and longitude λ is given by a pair of equations of this form

$$\cos \omega = \mathrm{A}+\mathrm{B} \sin l+\mathrm{C} \cos l \cos (\lambda+\mathrm{D})$$

$$t = \mathrm{E}+\mathrm{F} \sin \omega+\mathrm{G} \sin l+\mathrm{H} \cos l \cos (\lambda+\mathrm{K}).$$

Write these in the form

$$\cos \omega = \mathrm{A}+\mathrm{L}\{\sin l \sin l_0+\cos l \cos l_0 \cos (\lambda-\lambda_0)\} = \mathrm{A}+\mathrm{L} \cos \mathrm{PP}_0,$$

$$t = \mathrm{E}+\mathrm{F} \sin \omega+\mathrm{M}\{\sin l \sin l_1+\cos l \cos l_1 \cos (\lambda-\lambda_1)\}$$
$$= \mathrm{E}+\mathrm{F} \sin \omega+\mathrm{M} \cos \mathrm{PP}_1,$$

where P is the point (l, λ) and P_0 a point whose geocentric latitude and longitude are l_0 λ_0 given by the equations

$$\mathrm{C} \tan l_0 = \mathrm{B} = \mathrm{L} \sin l_0, \qquad \lambda_0+\mathrm{D} = 0,$$

and PP_0 is the arc of the sphere between P and P_0.

Similarly for the point P_1.

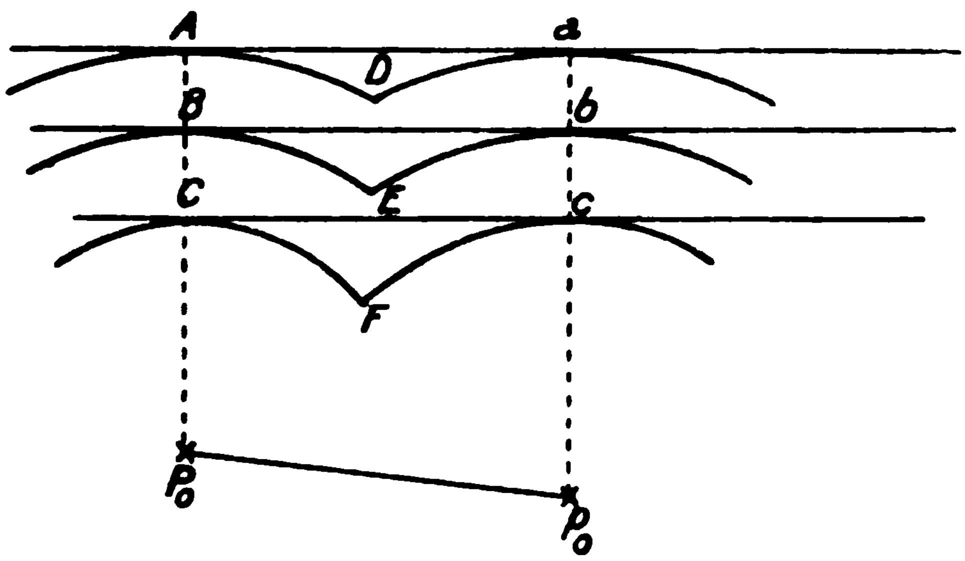
In the case of the second and third contacts, which determine totality, the equations only differ in the sign of F: so that the duration of totality is constant when ω is constant. The condition $\omega=0$ gives us points for which the eclipse is just total for an instant, *i.e.*, the points on the borders of the totality belt. But from the equation

$$\cos \omega = \mathrm{A}+\mathrm{L} \cos \mathrm{PP}_0$$

we see that ω is constant when the arc PP_0 is constant, *i.e.*, along the arc of a small circle described with centre P and radius PP_0.

The bounding lines of the totality belt given us by the approximate formulæ of the 'Nautical Almanac Circular,' are thus portions of small circles of the sphere.

Now, suppose we have formulæ given for the neighbourhoods of two points B and b on the central line; for which P_0 and p_0 are the centres of the approximate loci of equal totality. Then the approximate formulæ for B will give us as the northern boundary of the



totality zone the circular arc AD, touching the true line at A, but falling south of it elsewhere; and the approximate formulæ for b will give us the arc *ad*. Thus at the intermediate point D, both formulæ give errors in the same direction; and unless the points B and b are tolerably close together we cannot get a good prediction for intermediate points by simple interpolation.

The true lines A*a*, B*b*, C*c*, are the *envelopes* of such circles as AD, BE, CF, as we travel along the central line, P_0 travelling along the path P_0p_0 in correspondence.

It is to be noted further, that the approximate formulæ give a constant duration of totality along the line BE, which is the approximate central line (given by $\sin\omega = 1$): whereas the duration generally changes as we go east or west. But interpolation between results for B and b would probably give the means of allowing for this change.

If instead of the duration of totality, we take the time of one of the contacts, the approximate loci are no longer circles, but curves of the form

$$(a+b\cos PP_0)^2+(c+d\cos PP_1)^2 = 1,$$

and the geometry is less simple; but these curves will have the true line for their envelope just as the circles did.

Now, if the contact of the circles or the curves with their envelope is of the *second order* interpolation becomes possible, for the circles cross *the envelope and thus the* error introduced is + to the east

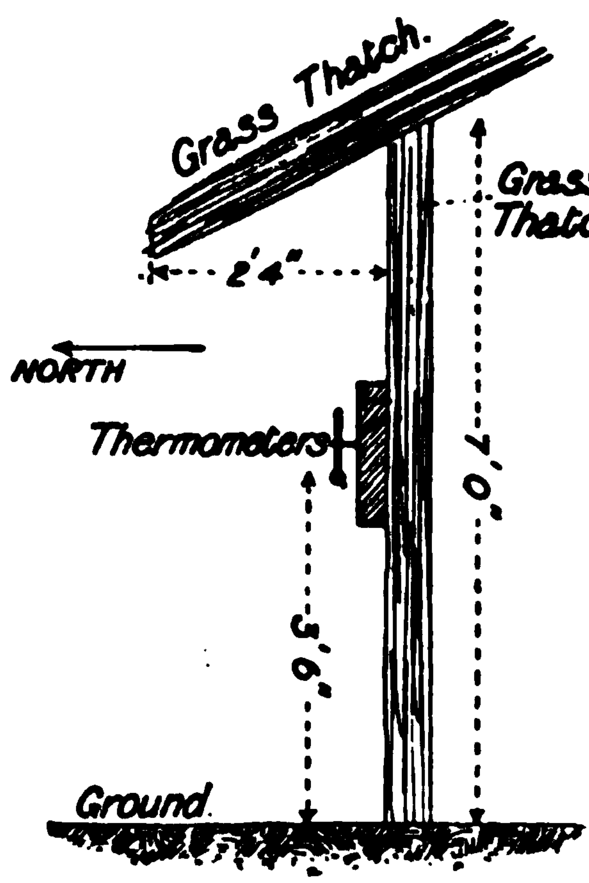
and — to the west. The test of this is to calculate the contacts for b with the constants given for B, and compare with true values for b; similarly calculate the contacts for B with the constants given for b and compare with the true values for B. If the errors are of opposite signs, then the curve crosses the envelope, and the contact is of the second order. A few experimental calculations of this kind, with the data of 'N.A. Circular, No. 16' seem to show, however, that the contact is not generally of the second order, and the approximations are thus subject to the disadvantages above indicated.

11. *Meteorological Observations.*—Lieut. Crosthwait, R.E., has kindly supplied the following particulars of the meteorological observations:—

They were made from 1898, January 14, to January 24 inclusive, by the following observers:—

Venayek Narayan, Narayan Vishnoo Apte, Govind Ramchandra Bhabhi, and Vishnoo Babaji Garnd.

The thermometers were attached to a board suspended to the north side of a grass hut, with an overhanging roof, completely shading them from the sun's rays at all times of the day. Height above ground and other dimensions shown in accompanying diagram.



The situation is open, facing towards the north, on a gently undulating plain, about 1500 feet above sea level. The only hills in the neighbourhood are from 2000—3000 feet high, distant about 12 miles.

The following instruments were used: A maximum and minimum thermometer No. 20,443 by Hicks; a wet and dry bulb No. 38,037 by Negretti and Zambra; a mountain mercurial barometer, standing on a tripod, No. 1824 by L. Casella.

Before beginning, the following comparisons were made with a standard thermometer No. 93,638 by Casella :—

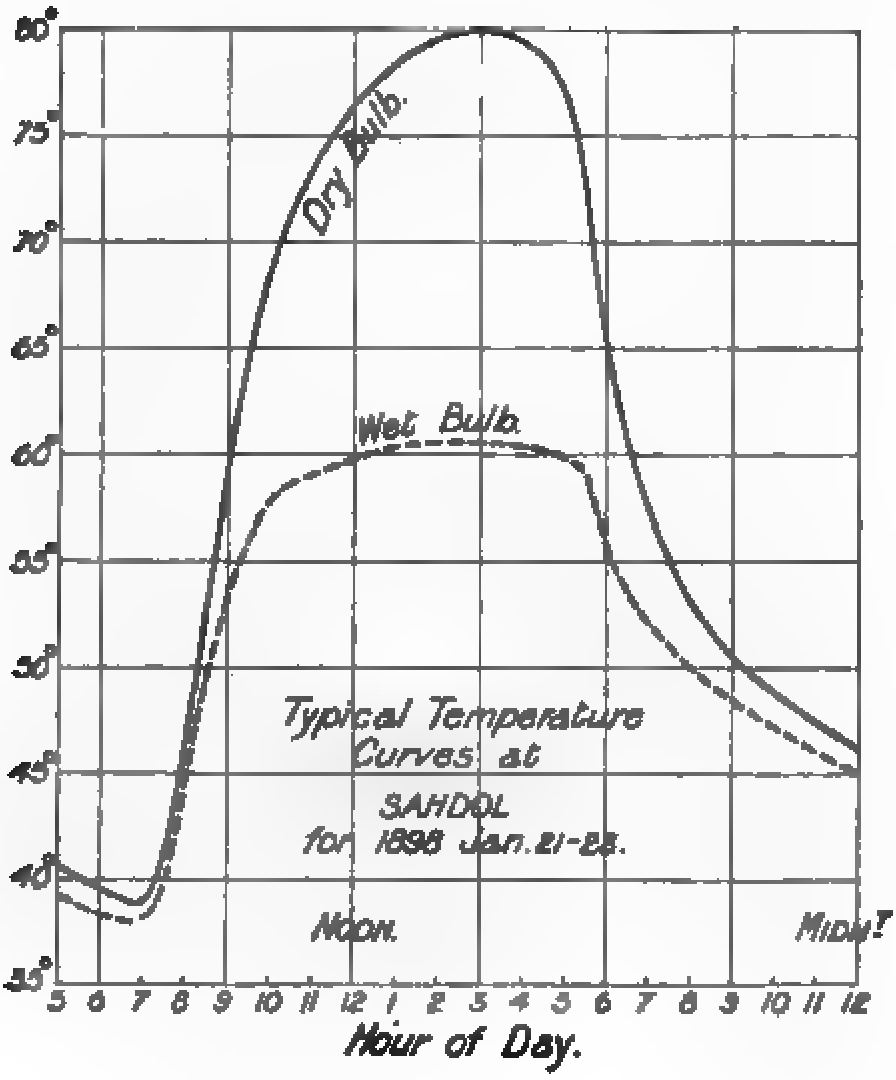
Standard read	78·6°		
Dry bulb "	77·9	correction	+0·7°
Wet " "	78·0	"	+0·6
Maximum "	78·0	"	+2·6
Minimum "	78·0	"	+0·6

The wet and dry bulb thermometers, and the barometer with its attached thermometer, were read every half hour from 5 A.M. till 8 P.M. or later. The maximum thermometer was read at 4 P.M., the minimum at 10 A.M.

On the day of the eclipse, observations were made of wet and dry bulb and barometer, and also of the standard thermometer (Casella No. 93,638) every five minutes.

The barometer was very steady throughout our stay, near 28·6 in.; and it seems scarcely necessary to publish the details.

The wet and dry bulbs both followed very closely the curves shown in the attached diagram, the deviations from the curve seldom exceeding a degree or two. To avoid needless printing, the follow-



ing particulars for a number of readings on January 21, 22 and 23 are given as illustrations:—

Excess of Readings of Thermometers over the Typical Curves shown in the Diagram.

	Dry bulb.			Wet bulb.		
Hour.	Jan. 21.	Jan. 22.	Jan. 23.	Jan. 21.	Jan. 22.	Jan. 23.
6 A.M.	−1·9°	−1·3°	+1·1°	−1·6°	−0·6°	+0·8°
8 "	−1·4	+0·5	+2·0	−1·8	−0·3	+1·8
10 "	−1·5	+1·5	+2·8	−0·8	−0·6	+1·1
NOON	−2·6	+1·3	+1·9	−0·8	+0·2	+0·8
1 P.M.	−1·6	0·0	+2·0	−1·9	−1·0	+2·1
2 "	−0·8	−9·0	+1·0	−1·6	−3·5	+1·5
4 "	−0·6	−2·6	+0·4	−0·3	−0·5	+1·5
6 "	+3·0	−2·3	+1·1	+0·2	−0·6	−0·9
8 "	−1·6	−0·6	+2·0	−1·6	+0·1	+1·8
10 "	−1·9	+0·3	+2·3	−0·8	+0·6	+1·8

[The anomalous readings of dry bulb at 6 P.M. on January 21, and of wet bulb at 6 P.M. on January 23, are apparently not mistakes, unless the neighbouring observations are similarly affected.]

We may thus take it that the typical curves of the diagram represent with considerable accuracy what would have been the state of things on January 22, if the eclipse had not taken place.

Comparing then the readings during the eclipse with this curve, we get the following differences, which may be regarded as the effect of the eclipse.

Effect of the Eclipse on Temperature as shown by Five-Minute Readings.

Time of day. hrs.	mins.	Dry bulb.	Wet bulb.
0	0 (NOON)	+1·2°	−0·1°
0	5	0·0	−0·7
0	10	+0·8	−0·2
0	15 First contact	+1·7	+0·2
0	20	+1·5	+0·1
0	25	+0·8	−0·9
0	30	+0·6	−0·5
0	35	+0·4	0·0
0	40	+0·3	−0·1
0	45	+0·2	−0·1
0	50	−0·5	−0·2
0	*55*	−0·6	−0·4

Effect of the Eclipse on Temperature as shown by Five-Minute Readings—*continued.*

Time of day. hrs.	mins.		Dry bulb.	Wet bulb.
1	0		−1·3	−0·8
1	5		−1·9	−0·8
1	10		−2·0	−1·3
1	15		−2·2	−1·4
1	20		−2·8	−1·9
1	25		−3·4	−1·9
1	30		−4·0	−2·5
1	35		−4·0	−2·5
1	40	Totality	−5·1	−3·5
1	45	Totality	−7·2	−3·5
1	50		−7·7	−3·5
1	55		−8·3	−3·6
2	0		−8·9	−3·6
2	5		−8·5	−3·1
2	10		−7·6	−2·6
2	15		−6·7	−2·2
2	20		−5·7	−1·9
2	25		−5·1	−1·7
2	30		−4·4	−1·7
2	35		−3·4	−1·5
2	40		−3·7	−1·4
2	45		−3·5	−1·2
2	50		−3·5	−1·2
2	55		−3·1	−0·6
3	0	Last contact	−3·1	−0·6
3	30		−2·6	−0·3
4	0		−2·5	−0·2

"Total Solar Eclipse of January 22, 1898. Preliminary Report on Observations made at Ghoglee, Central Provinces." By Professor RALPH COPELAND, Astronomer Royal for Scotland. Received May 10, 1898.

In the month of August, 1897, I was invited by the Joint Permanent Eclipse Committee to take part in observing the total solar eclipse which occurred in India on 22nd January of the present year.

The preparation of the equipment, which will be described further on, was at once proceeded with, and by the sanction of the University authorities and the Secretary for Scotland I was granted the

necessary leave of absence from the University and the Royal Observatory.

Having shared in the general disappointment of the Russian eclipse of 1887, and the no less unfortunate visit to Vadsö in 1896, I resolved on this occasion to occupy a separate station at some distance from any large group of observers. My only companion was our observatory engineer (James McPherson), who on the two previous occasions had shown his skill and energy in setting up and handling the instruments.

A grant of £180 was made to me by the Committee. This would have been amply sufficient had I dispensed with the services of Engineer McPherson, but without his aid I could not have carried out my plan of occupying an independent station and using several instruments.

Our equipment consisted of—

(1) A horizontal telescope of 38 feet focus and 4 inches aperture to be used with a fixed mirror, the image being received on 18-inch plates moved by clockwork. This instrument was provided with a direct vision prism mounted on a slide in front of the object-glass, which, when drawn into position by an attendant, transformed the telescope into a prismatic camera.

(2) A small prismatic camera designed for the investigation of the ultra-violet rays of the solar appendages. To this end the object-glass was of quartz and Iceland spar, the prism being of the latter material.

(3) A slit spectroscope by Grubb, with one compound prism. (2) and (3) were carried by a 4-inch equatorial mounting, and served to balance each other.

(4) A 4-inch camera with a doublet lens, by Dallmeyer, of 33-inch focus, mounted on a 3-inch equatorial stand. Both the equatorial mountings were fitted with driving clocks.

In place of a hut we were provided with a supply of laths, boards, and brown paper, plain as well as waterproof and blackened, for building the camera of the 40-foot, which had also to serve as a dark room. The hut built with these materials served the intended purpose satisfactorily. It was ventilated by a tin chimney like that of a magic lantern.

The instruments were despatched on the 20th November, *viâ* Leith and London, to Bombay. Engineer McPherson left Edinburgh on the 2nd December to embark in London on board the P. and O. s.s. "Britannia," while I, leaving on the 8th, was able to catch the same steamer at Brindisi on the 12th December. Bombay was reached on the 26th December, and Nagpur, in the Central Provinces, two days later. Here we were most hospitably received by Colonel Henry J. Lugard (Madras Staff Corps), and with his help

and that of his son, Mr. Edward Lugard, Executive Engineer, Bhundara, a station was finally selected on the last day of the year, close to the village of Ghoglee, 16 miles north-west of Nagpur, and about 2 statute miles south-east of the line of central eclipse. On New Year's Day I observed the sun for latitude, time and azimuth, and on the 2nd January we began our camp life, having lived meanwhile at Kalmeshwar, in a bungalow belonging to the Department of Works, three miles distant, on the way to Nagpur. We found the large double tent provided by the Government most comfortable. It was pitched beside a grove of mango trees, under which was a large well with a plentiful supply of water. On the 4th January concrete foundations were begun for the fixed mirror and lens, for the plate-carrier of the long-focus telescope, and for each of the two equatorials. The latter were placed in line with the dark room of the horizontal tube, so as to bring the observers within earshot of the metronome which was used to regulate the lengths of the exposures. It is needless to give particulars respecting the adjustments of the equatorials, which were considered sufficiently accurate when within two minutes of arc of the truth.

The method pursued with the horizontal telescope was as follows:—With the theodolite placed on the proposed centre line of the tube, the azimuth of a distant tree was found from observations of the sun, and also of the pole star. Two sets of footholes for the theodolite were made in blocks of cement at different levels, but referred to a common centre. By this means the theodolite could be used either as a collimator looking through the lens of the long telescope, or as a directing telescope to bring the tube, lens, and plateholder exactly into the required line. The fixed mirror was roughly adjusted as regards azimuth with the theodolite and a reflecting eye-piece, and as to inclination by means of a gauge and spirit level. The final adjustment was given by slightly turning the slow motion screws of the mirror until the sun's image ran along a parallel of declination drawn on the cover of the plate-holder, and also crossed one or other of a set of vertical lines at a given computed time. The tube being set exactly horizontal, the plate-holder was "squared on" by viewing the image of the object glass in a mirror held against the plate-holder in reversed positions. The long-focus object glass was "squared on" with the help of one of the little centering telescopes originally designed by Fraunhofer. The inclination of the slide on which the plate-holder ran was found from the computed altitude and azimuth of the sun's centre two minutes before and two minutes after mid-totality. The same data also gave the speed at which the plate had to move—in our case 1·872 inch per minute. *No provision was made* to counteract the very slight effect *of the rotation of the sun's* image during the period of exposure. The

sliding frame was drawn forward by a wire cord and weight, the rate of motion being regulated by the speed at which the driving clock "payed out" the cord. A tendency to advance by jerks was altogether removed by thoroughly strengthening the clock seat-board and its attachment to the fixed frame. The clock, kindly lent by Lord McLaren, was made by Sir Howard Grubb. We utilised the slide motion to obtain a chronographic record of the exposures. Three pencils, in holders hinged to the standing part and moved by strings, marked the time on a slip of paper pasted on the back of the moveable frame.

The distance of the plate-holder from the cell of the lens had been found in Edinburgh to be 455·27 inches. This interval was set off in India with light wooden rods, which were compared with a Chesterman steel foot-rule. The photographic focus of the Dallmeyer 4-inch had been found to agree exactly with the visual focus. The instrument was focussed in India with the help of a focussing glass, both in the day-time on an object at a distance of 440 yards, and on stars at night, in the former case making allowance for the divergence of the rays by means of the well-known formula for conjugate foci. The two results were practically identical, but differed considerably from the scale reading used at home, owing to the shrinking of the wooden tube, in which the grain runs crosswise. The tube of the Iceland spar camera, being made of well-seasoned teak, was found unchanged in length, as proved by the linear spectra of Sirius and γ Argûs.

The greatest difficulty was experienced in filling the plate-holders of the long-focus telescope. It was only after many hours spent in paring and rasping the twisted frames that the plates were adjusted and ready for exposure. Two plates were broken in the process.

Our programme was as follows:—Eight plates, 18 inches square, to be exposed by Engineer McPherson standing in the dark room, where he had control of the cord for opening the spring shutter, were provided for the horizontal telescope, each carried in a separate holder. The prism was to be pulled into position by our native butler, Vardhya, at a signal from McPherson.

Mr. Meehan, Assistant Engineer of the Public Works Department, had kindly volunteered to work the Dallmeyer camera. He was provided with nine quarter plates, mounted in three long slides. The shutter was of the ordinary pneumatic kind.

I took charge of the ultra-violet prismatic camera, with six plates in three reversible holders. I had also one plate (Cadett) in the Grubb spectroscope, which was to be exposed by simply drawing back the slide. Cadett plates were used for the horizontal telescope and the 4-inch, while partly Lumière and partly Cadett plates were to be tried in the prismatic camera.

Mr. Meehan came in good time on the morning of the 22nd in order to have a final opportunity of practising the management of the 4-inch. Everything being prepared to the best of our ability, I anxiously watched with the finder of the prismatic camera for the first contact. The first indentation in the sun's limb was not perceived until 11 secs. after the computed time, but as the telescope only magnified thirteen times, and there was a chance that the prediction might be slightly wrong, this agreement was considered satisfactory. Ten minutes before the beginning of totality the observers took their assigned stations, and a little later the metronome was set going 2 mins. 14·6 secs. before the computed total phase, McPherson saw the following edge of the diminishing sun's image exactly on the line which had been previously marked on the sliding frame. 20 secs. later he started the clock in accordance with a signal given by me, and after about a quarter of a minute had elapsed each observer made a mark on the chronograph slip in response to the measured "one, two, three," called out by me in time with the beats of the chronometer. From this moment, which was 40·8 secs. before the computed disappearance of the sun, I watched the shortening line of light with the finder. It seemed a long time in disappearing, but the sunshade was so dark that I felt sure that nothing but the photosphere could be seen through it. I therefore refrained from giving any signal until the last trace of light had disappeared. I then called out "totality," as previously arranged, and had the satisfaction of hearing the shutter of the 38-foot (which made a rather loud noise) closing at the end of McPherson's first exposure.

The whole of the eight plates were successfully exposed in the horizontal telescope.

	Exposure.	Object.
No. 1	1·4 secs.	Corona.
2	6·7 ,,	Corona.
3	3·8 ,,	Spectrum.
4	6·7 ,,	Spectrum.
5	9·5 ,,	Corona.
6	13·2 ,,	Corona.
7	5·4 ,,	Corona.
8	1·0 sec.	Spectrum.

The five photographs of the corona are much disfigured by an exhalation thrown off by the bass wood of which the plate-holders are made, which has caused the grain of the wood to print itself in broad streaks on the pictures. By combining the various negatives in one drawing, however, there is reason to believe that the details of *the brighter parts of* the corona can be satisfactorily *worked out. Plates 3 and 4* show very distinct spectral images

of the prominences corresponding to a number of lines in the violet region of the spectrum. On the last plate, which was exposed very shortly after the end of totality, there is a broad spectral band full of bright and dark lines of varying intensities, corresponding to irregularities in the sun's limb and the presence or absence of prominences. Many of the bright lines run out into the cusps. The scale of this photograph is such that H and K are 10 mm. apart. By an oversight the prism was not used with Plates 1 and 2 as was originally intended.

Mr. Meehan also exposed the whole of the nine plates allotted to him. Three of these show the coronal rays during totality. The fourth has the coronal streamers quite distinct, although the sun is already reappearing with a trace of Baily's heads. The fifth plate, taken several seconds later, shows the solar crescent still further disclosed, but with the whole of the moon's disc distinctly outlined against the background of corona. In the last plate there is nothing to be made out beyond the over-exposed solar crescent; the remaining three plates are blank. I am under great obligation to Mr. Meehan for his valuable assistance with this instrument, as well as for help in other directions.

In the small prismatic camera four plates proved to be as many as I could dispose of. The actual exposures attempted were 2·5 secs., 4·7 secs., 19·6 secs., and 14·0 secs. respectively. The resulting negatives show numerous rings and lines ranging from 1474 K to about W.L. 3000. The lines are sharpest in the first plate, while the 1474 K ring comes out more fully in the second and fourth negatives. The third plate is unfortunately blank.

The plate in the integrating spectroscope, which was used without a condensing lens, shows a spectrum of three strong bright lines with a number of feebler ones between them. With an exposure of about a minute, terminating just before the end of totality, the plate is decidedly under-exposed.

The makeshift chronograph, while working well for the other two observers, failed to record Mr. Meehan's signals distinctly, owing probably to the greater length of the cord required to reach his telescope.

On Wednesday, 26th January, we broke up our camp at Ghoglee and sailed from Bombay on the 29th, reaching Edinburgh on the 21st February.

I wish here to record my indebtedness to all the officials and private individuals, British and native, by whose kind aid the object of the expedition was so much furthered and our visit to Central India made so pleasant as well as scientifically interesting.

"Total Eclipse of the Sun, January 22, 1898. Preliminary Account of the Observations made by the Eclipse Expedition and the Officers and Men of H.M.S. 'Melpomene,' at Viziadrug." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

Received March 28, 1898.

LOCAL ARRANGEMENTS.

After various inquiries which I had made respecting the suitability of Viziadrug for observations of the total eclipse, I informed the Eclipse Committee that I was prepared to take charge of an expedition to that locality, and it was agreed that the observations at this station should be placed in my charge.

The latitude and longitude of the part of the fort at Viziadrug finally occupied were 16° 33′ 26″ N. and 73° 18′ 58″ E. respectively, and the duration of totality was estimated at 127 seconds.

In connection with the work at this station the Admiralty was asked for a ship of war to convey the observers from Colombo to Viziadrug, and to permit the use of the ship, if possible, as a base, to enable me to repeat the observations attempted in Norway in 1896 with the assistance of H.M.S. "Volage," which ship supplied twenty-four assistants during the eclipse and fifty volunteers for general observations.* As a result of the Royal Society's application, H.M.S. "Melpomene," in command of Captain Chisholm Batten, R.N., was told off to join the expedition.

The expedition, which left England on December 10, consisted of Mr. A. Fowler, Dr. W. J. S. Lockyer, and myself, together with the Marquis of Graham, who joined as a volunteer. Some little time after reaching Viziadrug Professor Pedler, F.R.S., joined the party from Calcutta, and shortly before the eclipse Mr. John Eliot, Meteorological Reporter to the Government of India, joined from Simla. On arrival at Colombo we found H.M.S. "Melpomene" waiting there, and at once proceeded to the selected spot of observation—Viziadrug.

During the three days' voyage to our station a call for volunteers was made by Captain Batten, and 120 came forward. Lectures and demonstrations were therefore at once commenced by Lieutenants Blackett, Colbeck, and Dugmore, second engineer Mountfield, and myself to the several parties of men who had undertaken to perform special pieces of work. Twenty-two separate groups of observers were formed. On our arrival at Viziadrug we were received very kindly by Mr. Bomanji, the collector of Ratnagiri, and an Overseer of the Public Works Department, who was on the spot in charge of

* 'Phil. Trans.,' A, vol. 190 (1897), pp. 1—21.

some most excellent masons and carpenters, picked men from Ratnagiri as we later ascertained, and plenty of material for the construction of the necessary concrete bases and huts. It was important to erect the huts as soon as possible, not only to shelter the instruments but the observers from the sun. Several screens were made which could be moved and placed in any required position; these were found to be invaluable while the instruments were being erected. A considerable number of coolies was also present to do such work as carrying packing-cases, sawing wood, clearing the camp, &c.

In the fort was also a police guard sent from Ratnagiri. The camp was watched both by day and night so effectively by them that no damage to any instrument was reported.

On the arrival of the "Melpomene" at Viziadrug, Mr. Bomanji came on board to report the arrangements which had been made for the expedition by the Government of India. As these were not quite completed, it was necessary for the first few days to return to the ship every evening, but afterwards Mr. Fowler, Dr. Lockyer, and myself took up our quarters at the Dak bungalow inside the Fort, close to the instruments. Meals were provided at the Collector's camp, which was also inside the Fort.

A party was landed at the fort on the afternoon of our arrival to inspect the site suggested by Mr. Bomanji, and it was at once evident that it would satisfy all requirements, provided the fluctuations of temperature of the great masses of masonry composing the fort had no disturbing influence on the steadiness of the air. In order to investigate this point a $3\frac{3}{4}$-inch telescope was erected, and observations of the surrounding landscape, and, at dusk, of various stars, were made, from which it appeared that the atmosphere was sufficiently steady for the observations.

Next morning the instruments were landed and the concrete bases for them were commenced. The erection of the huts was also begun by the native workmen and continued without intermission.

The instruments were set up as soon as their bases were ready, and by the end of a week all were practically in readiness for the eclipse. Constant clear skies enabled all the adjustments to be made without difficulty.

During the week preceding the eclipse the adjustments were frequently tested, and a complete system of drills was established.

As the number of volunteers was so large I pointed out to Captain Batten, who had volunteered to aid in a special branch of the work, the importance of his taking charge of the whole camp and giving all the necessary orders for conducting the operations during the general rehearsals, and the eclipse itself. He eventually agreed to this, and the procedure and time signals were arranged between us.

The groups of observers were as follows :—

1. Time.
2. 6-inch prismatic camera.
3. 9-inch „ „
4. Integrating spectroscope.
5. 6-inch equatorial.
6. Coronagraph.
7. Discs.
8. Sketches of corona without discs.
9. 3¾-inch equatorial.
10. Observations on stars.
11. Shadow-bands.
12. Meteorological observations.
13. Hand spectroscopes.
14. Prisms for rings.
15. Polariscope.
16. Landscape colours.
17. „ cameras.
18. Shadow phenomena.
19. Kinematograph for eclipse.
20. „ „ shadow.
21. Contact observations.
22. Observations on natives, animals, &c.

The observers were as follows :—

1. *Time Signals.*

Captain A. W. Chisholm-Batten, R.N.
F. Downton, Leading Seaman.
W. Woods, Yeoman of Signals.
W. Groves, Shipwright.
F. T. Marey, Private, R.M.L.I.
G. S. Fullilove, Private, R.M.L.I.
G. Cleary, Private, R.M.L.I.

2. *6-inch Prismatic Camera.*

Mr. Fowler.
Lieutenant O. de Wett, R.N.
C. Ironsides, G.M.
J. Turner, T.I.
F. Brading, A.B.
J. Innes, A.B.
G. Salt, Boy, 1st Class.

3. *9-inch Prismatic Camera.*

Dr. Lockyer.
Lieutenant Percival-Jones, R.N.R.
A. Ramage, A.B.
W. Bray, Ch. Arm.
A. Wilkins, Shipwright.
E. Ashford, A.B.
F. Fenton, A.B.
A. Carr, Boy, 1st Class.

4. *Integrating Spectroscope.*

Lieutenant G. C. Quayle, R.N.
J. Bird, Ch. E.R.A.
G. Travill, P.O., 1st Class.

5. *6-inch Equatorial with Grating Spectroscope.*

Sir Norman Lockyer, K.C.B.
Professor A. Pedler, F.R.S.
Mr. R. C. Steele, Gunner, R.N.
P. Ross, Ch. E.R.A.
G. Vanstone, Ch. E.R.A.
H. Brown, Ship's Steward's boy.

6. *Coronagraph.*

Staff-Engineer A. Kerr, R.N.
W. Holmes, E.R.A.
C. Moseley, Leading Stoker, 1st Class.
G. Collier, Stoker.

7. *Discs.*

A. Ruse, Ship's Corporal, 1st Class.
G. Pink, Qualified Signalman.
J. Henry, Boy, 1st Class.

B. Brook, Stoker.
A. McDonald, P.O., 1st Class.
A. Tull, Ship's Steward's Boy.

L. Pettingale, Leading Signalman.
W. Brooker, A.B.
S. Drew, Ordinary Seaman.

R. Sutherland, Leading Signalman.
W. Webb, A.B.
W. Corney, Stoker.

G. Price, A.B.
J. Jones, A.B.
F. Dibbins, Ordinary Seaman.

L. Gates, A.B.
R. Davis, A.B.
P. McKenna, A.B.

8. *Sketches of Corona without Discs.*

General: A. Richardson, P.O., 1st Class. W. Pankhurst, A.B. H. Lack, Boy, 1st Class.
N.E.: W. Anderson, A.B. E. Wilson, Ordinary Seaman.
N.W.: T. Wells, A.B. H. Brinstead, A.B.
S.E.: E. Dann. W. Evans.
S.W.: W. Clayton. A. Penny.

9. *3½-inch Equatorial.*

Sir Norman Lockyer, K.C.B.
Mr. H. Willmore, Assistant Engineer, R.N.
M. Moore, Stoker.

10. *Observations on Stars.*

Lieutenant Henry Blackett, R.N.
J. McDonald, A.B.
F. Stevens, A.B.
R. Buckland, Plumber's Mate.
T. Sutton, Stoker.
J. Fitzroy, Boy, 1st Class.
G. Russell, Private, R.M.L.I.

11. *Observations of Shadow-bands.*

Staff-Surgeon C. L. Nolan, R.N.
C. Hester, Private, R.M.L.I.
A. Purkington, 2nd S. B. Steward.

12. *Meteorological Observations.*

Mr. John Eliot, C.I.E., F.R.S.
J. Russell, Chief Stoker.
C. Butt, Leading Stoker, 1st Class.
H. Rockett, Stoker.
A. Wallace, Stoker.
G. Pratt, Stoker.
H. Wallburn, Stoker.
J. Bartlett, Stoker.
T. McCarthy, Stoker.
E. Perry, Stoker.
G. Woolston, Stoker.
G. Garrard, Stoker.
C. Mintram, Stoker.
P. Keefe, P.O., 1st Class.

13. *Hand Spectroscopes.*

Lieutenant C. E. B. Colbeck, R.N.
C. Kitchingham, Private, R.M.L.I.
C. Woodley, P.O., 1st Class.
P. Manning, Ordinary Seaman.
H. Mitchell, Stoker.
J. Dobson, Sergeant, R.M.L.I.

14. *Prisms for Observations of Ring Spectra.*
    - Mr. J. Mountifield, Senior Engineer, R.N.
    - W. Morris, E.R.A.
    - A. Howe, E.R.A.
    - C. Stacey, Leading Stoker, 2nd Class.
    - H. Knight, Leading Stoker, 2nd Class.
    - R. Coates, Stoker.
    - G. Tarrant, Stoker.
    - H. Warren, Stoker.
    - J. Inch, Stoker.
    - G. Gray, Chief Stoker.
    - J. Cross, Stoker.

15. *Polariscope.*
    - Staff-Surgeon C. L. Nolan, R.N.

16. *Landscape Colours.*
    - Lieutenant E. N. R. Dugmore, R.N.
    - G. Farrell, Boy, 1st Class.
    - W. Jacobs, A.B.
    - P. Darvil, Boy, 1st Class.
    - H. Rhodes, Ordinary Seaman.
    - H. Attree, Signalman.

17. *Landscape Cameras.*
    - Mr. Turner, Survey Department, Calcutta.
    - E. Gyngell, A.B.
    - H. Childs, Chief Stoker.
    - J. Collins, Chief Stoker.
    - J. Kearney, Leading Stoker, 1st Class.
    - E. Cross, Leading Stoker, 2nd Class.

18. *Shadow Phenomena.*
    - W. Keenan, Chief Carpenter's Mate.
    - A. Reynolds, Stoker.
    - W. Weeks, Shipwright.
    - G. Riley, Stoker.
    - B. Crunden, Stoker.
    - C. Carpenter, Stoker.

19. *Kinematograph for Eclipse.*
    - The Marquis of Graham.
    - A. Shilcock, E.R.A.
    - E. Green, Boy, 1st Class.
    - C. Thomas, Seedie.
    - P. King, Ordinary Seaman.
    - W. Cronen, Stoker.

20. *Kinematograph for Shadow.*
    - Mr. H. P. Barnett, Paymaster, R.N.
    - A. Gidney, E.R.A.

21. *Contact Observations.*
    - Lieutenant O. de Wet, R.N.
    - C. Ironsides, G.M.

22. *Observations on Natives, Animals, &c.*
    - W. J. C. Slocombe, Ordinary Seaman.
    - G. Whittingstall, Ordinary Seaman.
    - F. Beal, Ordinary Seaman.

Aides-de-Camp to Sir Norman Lockyer, K.C.B., F.R.S.
- Mr. W. H. P. Bourne, Midshipman, R.N.
- J. Hunt, P.O., 2nd Class.

The development of the photographic plates was commenced immediately after the eclipse, and it was found that the results were on the whole very satisfactory. No results, however, were obtained with the integrating spectroscope, and the kinematograph films taken by Lord Graham were too badly fogged to serve any useful purpose.

The dismantling of the instruments was commenced very soon after the eclipse, and the packing, together with the development and copying of the negatives, kept the party fully occupied until the morning of *January 25, when the* expedition left Viziadrug.

Half of the negatives and glass copies of the remainder were conveyed to England in charge of Mr. Fowler, while the remaining half of negatives and positives were sent home *viâ* Bombay.

The general time signals were given by a bugler under Captain Batten's orders. The chronometer was in charge of Lieutenant de Wet, R.N.

For the work of the prismatic cameras it was important to get a signal as nearly as possible five seconds before the beginning of totality, and, in order to eliminate the possible error of the chronometer, it was arranged to determine this by direct observations. Two methods were adopted. In one of them a boat was moored at a distance of two miles from the camp, in the direction of approach of the shadow, which would pass this point five seconds before totality. This failed because of the indefinite boundary of the shadow.

The other method was to determine when the visible remaining crescent subtended an angle of 45°; calculation showed that this would occur at the desired interval from totality. This method was completely successful.

The special signals during totality were given every ten seconds, beginning at 127—the assumed period of totality—by means of the eclipse clock (which was started at the signal "go" by cutting a thread thereby releasing the pendulum), by two timekeepers, one during the first half, the other during the second half of totality.

In the system adopted not only was the time left called out every tenth second, but other signals were interpolated to guide the work in the photographic huts. In order that there might be no mistake about the calls, a spiral was drawn on the clock-face and the seconds left plainly marked at the points which the second hand would occupy during its two revolutions.

In consequence of the perfect drill acquired at the rehearsals the operations went off during the eclipse with absolute steadiness. They commenced about one and a half hours before totality, ending after a like interval after totality. Six volunteers were employed in the timekeeping, including three with lamps which were not wanted.

The Chief Instruments employed.

The Prismatic Cameras.

In the two prismatic cameras no less than fifty-seven photographs were secured, the exposures varying from one to fifty seconds. Such a result as this could only be obtained by a minute subdivision of labour. In the case of each of these two instruments six volunteers were employed, and they were distributed in the following manner:—

One observer with the finder, his duty being to keep the image

in the centre of the field of view which corresponded (by previous adjustment) to the centre of the plate in the prismatic camera. He had a timekeeper to record the times of contact.

A third acted as timekeeper to record the exact moments at which the exposures were begun and ended.

A fourth volunteer, by means of a piece of cardboard, covered and uncovered the front of the prism, from directions given by Mr. Fowler and Dr. Lockyer respectively.

In one case two, and in another three, men were required to hand and receive the large dark slides before and after exposure, taking them out or placing them back in bags made for this purpose.

Six-inch Prismatic Camera.

This instrument, the dispersion of which had been increased this year by the addition of a second prism, was worked by Mr. Fowler, with the assistance of Lieut. de Wet and five men. Mr. Fowler's programme was to begin taking a series of ten snap-shot pictures five seconds before the commencement of totality, to obtain a record every second or thereabouts of the spectrum of the chromosphere. After this he exposed eight other plates to secure photographs of the coronal rings, the exposures being of various lengths. It was also arranged that at five seconds before the end of totality he should commence another series of ten snap-shots, exposing the last of these some few seconds after totality. On developing the plates it was found that everything had gone satisfactorily. The large plates containing the ten snap-shots give the whole story of the chromosphere during twelve seconds, the time to make the ten exposures, and in one of the negatives there are as many as a thousand lines (about).

The last set of ten exposures did not come out quite as expected, for the reason that the duration of totality was a few seconds shorter than had been provided for in the time table, so that only two of the exposures were made before the end of totality. The very last exposure, however, taking about nine seconds after totality, shows many bright lines.

Nine-inch Prismatic Camera.

This instrument was in charge of Dr. W. J. S. Lockyer, who was assisted by Lieut. Percival Jones, R.N.R., and six men. This instrument was also fed by a siderostat, but the tube was not placed horizontally. It was intended with one of the prismatic cameras to so mount the tube that the arcs formed on the photographic plate should be symmetrical about the direction of dispersion, and it was decided that the 9-inch camera should adopt this plan of mounting.

The exact position of the tube to obtain this result was carefully determined by calculation. To facilitate the erection of the instrument at the station two wooden tops to carry the tube were previously made and taken out.

It is satisfactory to state that the photographs showed that the experiment was very successful, the arcs coming out exactly as forecasted.

Although this instrument was capable of only giving about half the dispersion of the 6-inch, the optical parts were better adapted for recording the ultra-violet region of the spectrum.

The programme adopted was similar to that of the 6-inch, there being two large plates ($16 \times 6\frac{1}{2}$) for recording a series of ten snapshots at and near the times of second and third contacts and nine smaller plates for exposure during totality. All the exposures were successfully made, but the lines in the spectrum are not so distinct owing to warping of the wooden tube by the heat and the consequent disturbance of the focus.

I shall refer to the results obtained by the prismatic cameras later in this preliminary report.

Integrating Spectroscope.

This instrument consisted of a large collimator, two prisms of 60°, and a receiving camera. It was entrusted to the care of Lieut. G. C. Quayle, R.N., with two assistants. The light which fed this instrument was obtained from a cœlostat, and there was still sufficient room for another instrument to be utilised, so the coronagraph was set up in the same hut. Although three exposures were made, no results were secured owing, it is feared, to an alteration of the slit, which was found closed after the eclipse.

Six-inch Equatorial with Grating Spectroscope.

This instrument consisted of a 6-inch lens mounted equatorially. The small grating employed contained 17,296 lines to the inch, and in the focus of the eyepiece was placed a small photographic spectrum of iron for comparison.

Professor Pedler, who came to take charge of this instrument was assisted by Mr. Steele, R.N., gunner, and three other volunteers. Up to the present time I have not received Professor Pedler's report of his observations, but I may say that among his observations reported at the time, he recorded the presence of arc lines of iron in the lower corona and the absence of the enhanced lines.

The Coronagraph.

All the more important instruments available for the expedition being employed in the spectroscopic work I could only use a small one for taking photographs of the corona, which were essential for me in order to make comparisons with the chromosphere and coronal rings we hoped to get in the prismatic cameras. The instrument employed, of $4\frac{5}{8}$-inch aperture, was entrusted to Staff-Engineer A Kerr, R.N., who was assisted by three volunteers.

Five photographs were taken. These on development were found to be exceedingly good, the long exposed plate showing a great amount of detail both in the polar rifts and in the streamers.

There being still a small amount of available surface of the cœlostat for other purposes, this was utilised for the 10 × 8 landscape camera which was operated by Mr. Turner. Two exposures were made during totality, with very successful results. The longest exposure shows very well the general form of the corona and the relative lengths of the extensions, the longest streamer being nearly three lunar diameters.

Discs.

The discs, six in number, were put into position by Lieutenant G. C. Quayle, R.N., and Lieutenant C. E. B. Colbeck, R.N., being ranged along the southern wall of the fort, close to the Eclipse Camp. The great altitude (53°) of the sun rendered the operation of setting them up somewhat difficult. Their sizes varied from six to two inches, and they were so placed that they cut off 3, 5, and 7 minutes of arc round the dark moon.

Each disc occupied the time of three men, so that in all eighteen volunteers were employed. Of each party of three, one volunteer kept the eye end in adjustment up to the time of totality, another who was blindfolded ten minutes before totality acted as observer, and the third wrote down the remarks of the observer.

A preliminary examination of the drawings shows that no equatorial extension was observed.

The $3\frac{3}{4}$-inch Equatorial Telescope.

This telescope was used by me to observe the exact time of second and third contacts to give the signals "go" and "over" to the timekeepers. For the first fifty seconds of totality I employed this instrument myself to minutely observe the structure of the rifts and streamers. In my absence it was used by Assistant Engineer H. H. Willmore for the examination of the structure of the corona.

Observations of Stars during Eclipse.

This party of six volunteers was in charge of Lieutenant Henry Blackett, R.N. Each observer was supplied with a photograph of a small star chart of the region near the sun, prepared by Dr. Lockyer. This was afterwards supplemented by another on a larger scale photographed at the office of the Trigonometrical Branch of the Survey of India at Dehra.

Two striking observations were made by most of the observers. First, more stars were seen just before the commencement of totality than during the actual period of totality; that is, they were logged as disappearing just before the total eclipse phase commenced. A similar observation was made by Admiral Don Ulloa in the eclipse of 1778.* Secondly: two observers noted on the chart a bright body, certainly not a star, midway between the planets Mars and Venus. It was seen only for a short time, and that before totality, being estimated as of the Second Magnitude.

Meteorological Observations.

Mr. Eliot, the Meteorological Reporter to the Government of India, brought with him several important instruments with a view of making observations similar to those he had arranged along the whole line of totality. The report of his observations I have not yet received. He was assisted by twelve volunteers.

Observations of Shadow Bands.

Staff-Surgeon Nolan, R.N., observed these phenomena with the help of two assistants. Previous to the eclipse a large white tablecloth was spread on a flat piece of ground in front of two walls intersecting at an angle of 115°, which were whitewashed. The bands were well seen before the second and after the third contact. None were seen during totality. Their direction of travelling was before totality towards the west (N. 88° W.), veering gradually round to S. 60° W. After totality they practically reversed their direction, travelling N. 60° E. They moved too quickly for their rate of motion to be determined, but it was noted that their rate of motion was not constant.

They were estimated to be about ½ to 1½ inches in breadth, but this also varied. The interspaces were gauged at 4 to 6 inches in breadth.

Each observer noted, one minute after totality, a long intermittency during which a large band, about 2 inches broad, passed by itself in a most striking manner.

* 'Phil. Trans.,' 1779, p. 105.

The Chief Results bearing on Solar Theory.

1. *The Spectrum of the Chromosphere.*

Considerable time must elapse before the complete discussion of the numerous photographs taken in the prismatic cameras can be completed. I therefore give here only some general results which can be gathered by a preliminary inspection of them.

I first deal with the determination of the heights of the various absorbing vapours so far as they can be gathered from the photographs, which, of course, only record for us the brightest lower portions of the different arcs, and not their complete extension.

The following table shows the results obtained in the case of some of the most typical lines:—

Lines.	Length of arcs.	Height.	
		In miles.	In secs. of arc.
Ca(K)	130°	6000	13·3
Hydrogen	112°	4500	10·0
He 4471·25	105°	4000	8·9
He 4026·3; Sr 4077·9, 4215·66	86°	2700	6·0
Ca 4226·9; Unknown, 4247 Mg ultra-violet triplet	72½°	2000	4·4
Fe triplet (4045) Strongest arc lines (4307·96, 4325·92, &c.) Al 3944·16 and 3961·67	60°	1450	3·2
Fe enhanced lines 4584, 4233 Mn quartet (4030·9, &c.)	51°	1100	2·4
Fe enhanced quartet (4523·0, &c.) and many other lines	40°	650	1·4
Carbon fluting and many lines, including arc lines of iron	35°	475	1·05

A very noticeable feature of the chromospheric spectra, which the photographs enable us to investigate at different elevations, is the difference in the behaviour of the gaseous and metallic lines. In the spectrum taken very near the moment of second contact, representing that of the lower strata with the spectra of higher ones superposed, the metallic arcs are relatively short and very bright, while in later photographs, representing the spectra of successively higher strata free from admixture with lower ones, the metallic arcs are relatively feeble. This is also indicated in another way by the varying effects seen over the tops of lunar mountains and through indentations in the moon's limb.

Some of the lines are seen to be relatively much brighter in the

upper strata than in the lower, such lines showing no notable increase of brightness at the points where lower strata are revealed through lunar valleys. Chief among these lines are those of hydrogen, helium, and calcium (H and K), but there is an additional line at wave-length 4686·2 or thereabouts, which behaves in the same way.

This line does not appear in Young's list of chromospheric lines, and all attempts to trace it in known spectra have failed. A line apparently coincident with it, however, has been found in the photographed spectrum of a tube containing helium, which is one of a series of comparison spectra being taken with the 6-inch prismatic camera to facilitate the reduction of the eclipse photographs.

The only recognised impurity in the vacuum tube used is oxygen, but besides the line to which reference has been made, there are a few faint lines for which no origins can at present be assigned.

It is worthy of remark that this line falls very near to the first line of the principal series in the spectrum of hydrogen, recently calculated by Rydberg to have a wave-length of 4687·88.*

As in the case of the photographs taken with the prismatic cameras in 1893 and 1896, the spectrum of the chromosphere in 1898 is very different from the Fraunhöfer spectrum, so that we have not to deal with a mere reversal of the dark lines of ordinary sunlight into bright ones. (See fig. 1, next page.)

Many very strong chromospheric lines, as the helium lines for example, are not represented among the Fraunhöfer lines, while many Fraunhöfer lines are absent from the chromospheric spectrum.

2. *The Spectrum of the Corona.*

The heights of the chief coronal rings as photographed are roughly as follows:—

1474 K	60,000 miles (in lower parts of inner corona).
3987·4	20,000 miles.
4231·3	More than 10,000 miles.

The coronal rings not only differ from the chromospheric ones in regard to the heights to which they extend above the photosphere, but also in appearance.

The outlines of these rings are distinctly not connected with the configuration of the chromosphere and prominences. In photographs taken near the beginning and end of totality, the 1474 ring is brightest on the same side of the moon, although the chromosphere and prominences are first visible on one side and then on the other. None of the rings give any indications of increased brightness at the places occupied by prominences. The green ring, corresponding to

* 'Astro. Phys. Jour.,' vol. 6, p. 237.

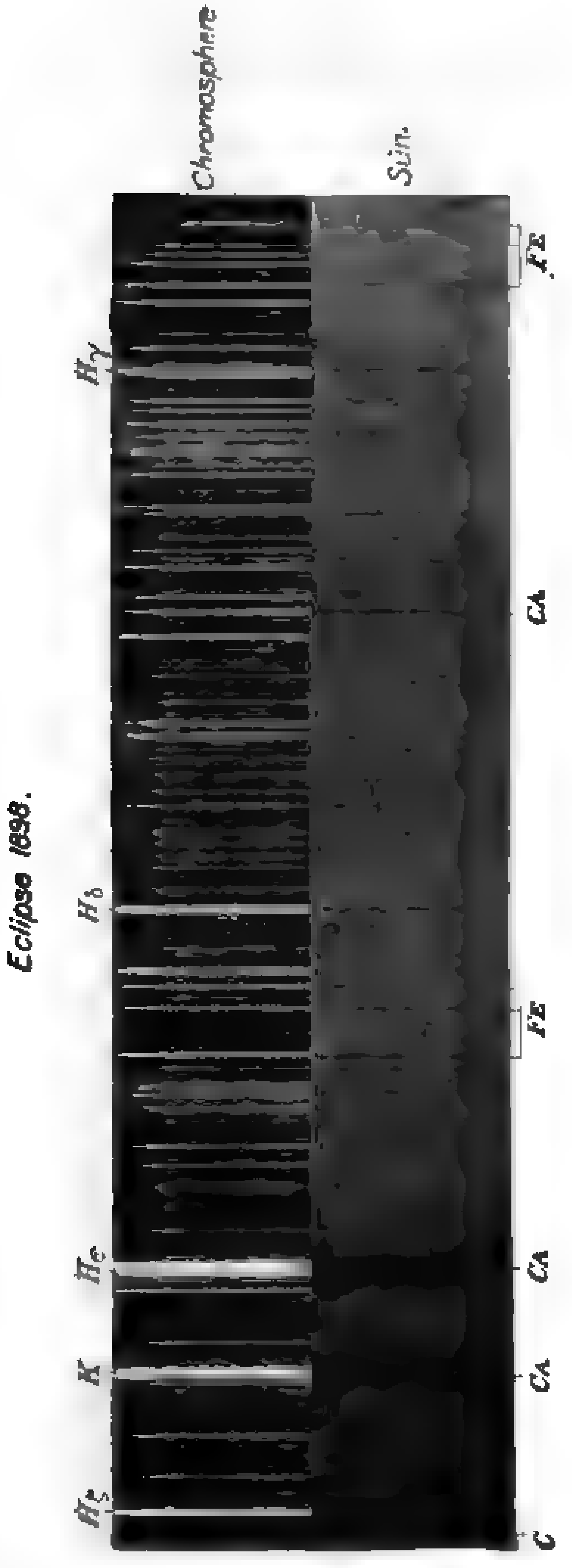


FIG. 1.—Spectrum of Chromosphere as photographed during Eclipse compared with Fraunhofer lines.

1474 K, which is the brightest of the rings seen, can be traced completely round the limb, and while in some parts it is very feeble, in others it is bright enough to show the brightest projections of the inner corona as photographed with short exposures with the coronagraph. The other rings at 3987 and 4231 can also be traced completely round the limb, but they are fainter on the average and of much more uniform intensity than 1474 K. This latter fact suggests that the additional rings are produced by a substance which is not the same as that to which 1474 K corresponds.

It is interesting to note that the three rings photographed in 1898 were also the most conspicuous in the coronas of 1893 and 1896 as determined by the use of prismatic cameras. The following table gives a comparison of the results obtained in the three eclipses, the wave-lengths for 1898 of course being only provisional.

1893.	1896.	1898.
3987	3988	3987·4
4086	4084	
4217		
4231	4232·0	4231·3
4240		
4280		
4486		
5316·9	5316·9	5316·9

3. *Results regarding the Corona.*

I looked forward to the corona this year with the greatest interest on account of the high temperature of the sun as judged by the fact that scarcely any iron lines have been recorded as most widened in the spectrum of sun spots since the end of 1892; that is, chemically, the maximum sun-spot conditions have been retained since 1893. Hence I was not astonished to see several large spots on the sun on the days preceding the eclipse.

I pointed out in 1878, a year of minimum, that the corona of that year was vastly different from that of 1871, a year of maximum; not only was it very much dimmer, but its spectrum was continuous; there were practically no bright lines, while long equatorial extensions were seen.

Normally we should have expected an approach to the 1878 conditions this year. But both the photographs and eye observations show that the only minimum appearance noticeable was the exquisite *tracery near the* sun's poles.

The violent magnetic storm and bright aurora on March 15 and 16,

to which Mr. Chree has recently called attention, follow suit with the chemical and eclipse observations, and it is important to note, as Dr. Chree has informed me in a later communication, that there were less violent disturbances on January 15—18 and February 11—16, so that there have been three disturbances separated roughly by an interval of twenty-eight days.

CONCLUSION.

The extraordinary interest and the skill displayed by the officers and men of H.M.S. "Volage" under Captain King Hall in 1896, and of H.M.S. "Melpomene" under Captain Chisholm Batten in the present year, prove beyond all question that in eclipses in which a man-of-war can be employed the most effective and the most economical means of securing observations is to depend upon the naval personnel, one or two skilled observers being sent out to help in the final adjustments of instruments according to the number it is intended to employ.

At Viziadrug, Mr. Fowler and Dr. Lockyer were enabled to report all the fixed instruments and huts, eight in number, erected and all but the final adjustments made after six days' work, a long break being necessary in the middle of the day in consequence of the heat. Such an achievement as this is beyond all eclipse precedent and was only rendered possible by the help of a large staff of highly trained men. Of the 150 engaged in the operations only three originally formed the expedition.

It is, therefore, quite inappropriate that I, on the part of the expedition, should here tender thanks to Captain Batten, the officers and men of H.M.S. "Melpomene" for their assistance, for as matters turned out we assisted them; but we are anxious to place on record the kindness we received from them both afloat and ashore, and since the great success of the recent observations is due almost entirely to Captain Chisholm Batten and the ship's company of the "Melpomene," I trust that the President and Council of the Royal Society may be pleased to communicate this fact to the Lords Commissioners of the Admiralty.

Among those to whom thanks are specially due are the following representing the Indian Government:—

E. Giles, Esq., Director of Public Instruction, in charge of arrangements made by Bombay Government.
K. R. Bomanji, Esq., Collector of Ratnagiri.
J. L. Jenkins, Esq., Collector of Salt.
E. H. Aitken, Esq., Assistant Collector of Salt.
F. R. Bader, Esq., Assistant Engineer, P.W.D.

Gangadhar Anant Bhat, Executive Engineer, P.W.D.
Govind Goshi, Overseer, P.W.D.
Sadashi Govind Joshi, Clerk to the Overseer, P.W.D.

Thanks are also due to the Officers of the Police, Telegraph, and Customs Departments, and others representing the Bombay Government, for their unceasing efforts to help us in every way.

Everybody was struck by the admirable and smart manner in which the subordinates of the Public Works Department accomplished their respective tasks.

I took upon myself when leaving Viziadrug, to write an unofficial letter to Mr. Bomanji, thanking him, in the name of the expedition, for his great personal kindnesses to us as well as for the valuable assistance we had received from him and the other local representatives of the Government.

L. Lee, Esq., Collector of Customs for Ceylon, and other Customs officials at Colombo rendered valuable assistance to the expedition by granting special facilities and providing means for transhipping the instruments.

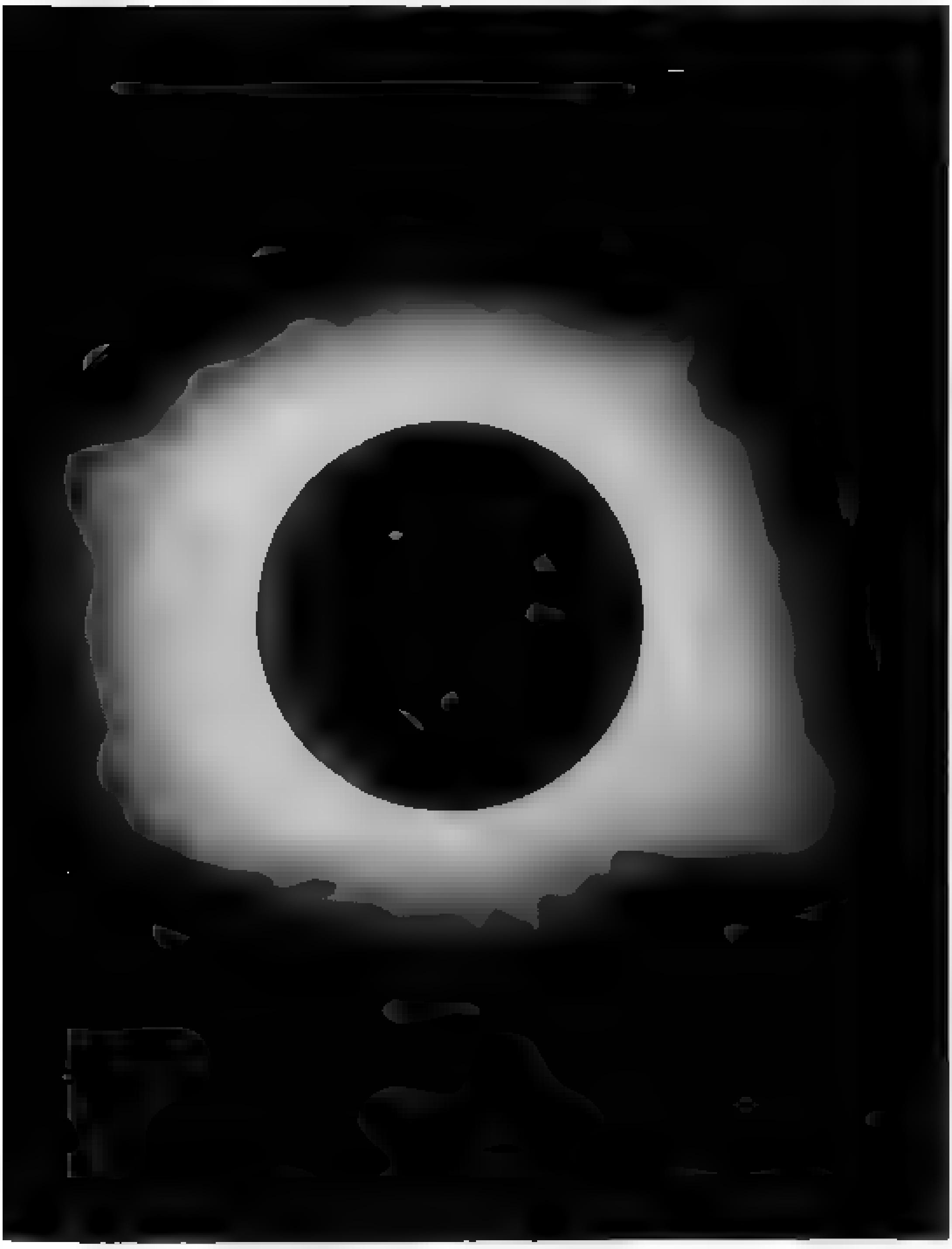
The Orient Steam Navigation Company very kindly conveyed the instruments free of charge to and from Colombo.

To W. H. Sinclair, Esq., a former Collector of the district (now retired), I was indebted for the supply of much valuable local information before leaving England.

My own personal thanks are due to Mr. Fowler and Dr. Lockyer, who assisted me in the preliminary work of organisation, and who, while at Viziadrug, worked hard both day and night to further the objects of the expedition; and also to Mr. Bourne, Midshipman, attached to me as Aide-de-camp, who was indefatigable in helping me to carry out the various details of the local organisation.

THE SOLAR CORONA

OF 1898.—JAN. 22ND.



Photographed at Pulgaon, Central India, by CAPTAIN E. H. HILLS, R.E.
Dallmeyer Photoheliograph Lens, 5 in. diameter.— Equivalent focus, 15 ft.
Exposure 8 seconds.

"Total Solar Eclipse of 1898, January 22. Preliminary Report on the Observations made at Pulgaon, India." By Captain E. H. HILLS, R.E., and H. F. NEWALL, Sec. R.A.S. Received May 25, 1898.

(PLATES 1—3.)

CONTENTS.

I. *Origin of the Expedition and General Preparations.*

By Captain Hills and H. F. Newall.

This expedition was one of those organised by the Joint Permanent Eclipse Committees of the Royal Society and Royal Astronomical Society, funds being provided from a grant made by the Government Grant Committee.

The observers are indebted to the Great Indian Peninsular Railway Company for the carriage of the instruments at reduced rates between Bombay and Pulgaon, and for a considerable reduction of fares to the observers for this journey.

Observers.—The party consisted of :—

Captain E. H. Hills, R.E., Instructor in Chemistry and Photography at the School of Military Engineering, Chatham.

H. F. Newall, Sec. R.A.S., Cambridge Observatory.

(In what follows these will be designated by the initials H. and N.)

It had originally been arranged that Dr. E. J. Stone, Radcliffe Observer, Oxford, should be a member of the party. The vacancy caused by his lamented death was not filled, as it was decided to use the skilled assistance which could be obtained locally in order to carry out part of the programme of work that Dr. Stone intended to attempt, namely the obtaining of twelve photographs of the corona with the double tube camera.

Local Arrangements.—When the preparations were being made for this expedition the Surveyor-General of India intimated that his department would be willing to give what assistance they could. This generous offer was gladly accepted by the Joint Permanent

Eclipse Committee, and the Surveyor-General was asked if he could send—

(*a*) An officer who would take general charge of the camp.

(*b*) Six skilled native assistants.

(*c*) A photographer who would bring with him a suitable dark room ready for erection, and photographic materials.

The officer detailed to take charge of the camp at Pulgaon was Captain G. P. Lenox Conyngham, R.E., and the observers feel that they owe much of the success of the expedition to the excellence of all the arrangements made by him.

The thanks of the observers are also due to Lieut.-Colonel St. G. Gore, R.E., Superintendent of the Trigonometrical Survey, for the continuous interest he took in the work, and to Lieut. G. A. Beazeley, R.E., for much help in the observations, and in the developing and copying of the photographic plates.

The observers are also indebted to the local authorities for their kindness in doing everything that was possible to render the time spent in the Eclipse Camp, both pleasant and profitable, in particular to W. A. Nedham, Esq., Commissioner, Nagpur; S. N. Chitnavis, Esq., Deputy Commissioner, Wardha; and A. C. Blennerhassett, Esq., I.C.S., Assistant Commissioner, Wardha. A number of others, whose names are mentioned below, took part in the actual observations, and the observers wish to express their grateful thanks for the valuable assistance thus rendered.

Selection of Station.—In order that the masonry piers to carry the instruments might be built, and that all the arrangements for forming the camp might be proceeded with before the arrival of the observers it was considered advisable that the choice of the actual station, the approximate position of which had been already decided upon, should be left to the Survey officer in charge. The place selected was Pulgaon, on the Nagpur branch of the Great Indian Peninsular Railway, and the camp and observatory were placed on an open piece of ground about a mile to the north of the station. The position proved excellent in every way.

Arrival at Station.—N. arrived at the camp on January 10, H. on January 12. All the instruments, which had been forwarded direct from Bombay, had previously arrived, and the necessary piers and huts for the observatory were found completed in accordance with the plans prepared and sent by the observers to Captain Lenox Conyngham. It was thus possible at once to proceed with the erection and adjustment of the instruments.

Meteorological Observations.—A continuous set of meteorological observations were made from January 16 to January 23, of which it may be interesting to give a summary.

	Wind.	Barometer.			Temperature in shade.								Maximum in sun.	Minimum on grass.
					Dry bulb.			Wet bulb.			Max.	Min.		
		9 A.M.	12 NOON.	3 P.M.	9 A.M.	12 NOON.	3 P.M.	9 A.M.	12 NOON.	3 P.M.				
Jan. 16	Very light, N. to N.W. ..	29·2	29·4	29·2	66	90	88	58	72	67	95	44	110	42
„ 17	Very light, E. to N.	29·2	29·3	29·2	68	87	89	58	69	69	95	43	110	39
„ 18	Calm	29·2	29·4	29·3	65	86	92	56	69	73	93	41	109	37
„ 19	Calm	29·3	29·4	29·3	68	83	86	58	66	66	89	40	105	36
„ 20	Calm to very light, S.E...	29·2	29·3	29·2	64	82	86	55	65	69	90	43	106	39
„ 21	Calm to light, S.E.	29·2	29·3	29·2	65	86	90	57	69	71	94	46	110	42
„ 22	Calm	29·2	*	29·2	69	90	88	59	73	71	92	45	107	41
„ 23	Calm	29·3	29·3	29·3	70	86	93	62	67	74	96	48	113	43

* Eclipse day. Barometer not read at noon.

Total rainfall, nil. Total cloud amount, nil.
The instruments employed were standard ones by Negretti and Zambra.

It is interesting to compare these figures with those given by Mr. Eliot in his meteorological note prepared in connection with the eclipse. No data are given for Pulgaon, but the conditions are practically the same as those found at the two nearest stations for which the figures are given, namely Akola and Nagpur.

We have then—

	Temperature.			Cloud amount (parts in 10).		Rain-fall.
	Max.	Min.	Daily range.	10 A.M.	4 P.M.	
Average at end of January in a few recent years—						in.
Akola	84	53	31	1·00	1·48	0·11
Nagpur	83	55	28	1·41	2·18	0·14
Observed—Pulgaon	93	44	49	Nil	Nil	Nil

The figures exhibit the futility of selecting an eclipse station on meteorological data only.

Departure.—The observers left the camp on January 25.

II. *Totality at Pulgaon.*

By Captain Hills and H. F. Newall.

The preparations for totality as regards the instruction and drilling of the assistants calls for little mention. The skilled native assistants, provided by the kindness of the Surveyor-General, were thoroughly accustomed to observing work, and the preparations and preliminary drills proceeded with the utmost smoothness.

The two men selected as timekeepers were instructed to call out the seconds from the beats of a metronome, which had been previously carefully rated.

The signal for the beginning of totality was given by Captain Lenox Conyngham who was making the exposures with the double tube camera, but it was also necessary for the spectroscopic work to get a signal at some definite time before totality which was accomplished by the following method:—

The length of the diminishing crescent of the sun was calculated for 15 seconds before totality. The observer in charge of the double tube camera watched the image on his ground glass and gave a signal when the crescent had arrived at the calculated length. The actual interval between the 15 second signal and the beginning of totality was 13 seconds.

Observing Party.—The following is a complete list of the whole observing party :—

Double Tube Camera.

In charge of instrument—Captain G. P. Lenox Conyngham, R.E.
Exposer—Babu S. C. Goha.
Recorder—Babu S. N. Saha.
Handing slides—Kali Din.
Receiving ditto—Mahabri.
Holding bags—Balgar.

Spectroscopic Cameras.

Observer—Captain E. H. Hills, R.E.
Exposer—Quartz spectroscope, Lieutenant F. R. H. Eustace, R.E.
" —Flint spectroscope, Babu I. C. Dev.
Assistant in charge of slow motion—Lieutenant G. A. Beazeley, R.E.
Recorder—Mrs. Hills.

Spectroscope with Two Slits.

Observer—Mr. H. F. Newall.
Assistants—Mrs. Newall, Babu S. B. Shome.

Grating Spectroscope.

Observer—Mr. H. F. Newall.
Assistant—Mr. A. C. Blennerhassett, I.C.S.

Time Keepers.

Sub-Assistant Superintendent Hanuman Prasad.
Babu Lal Singh.

Recording Thermometers.

Captain G. C. Kemp, R.E.

Observing Magnetometer.

Lieut.-Colonel St. G. Gore, R.E.

Photographing Shadow Bands.

Mr. J. Harrold.

Recording Contacts.

1st and 4th—Captain Hills, R.E.
2nd—Captain G. P. Lenox Conyngham, R.E.
3rd—Mr. E. Batchelor, I.C.S.

Observed Times of Contacts.

Pulgaon—Latitude, 20° 44′ 10″·6 N.
Longitude, 78° 19′ 2″·5 E.

Computed distance from centre line, 4 miles.

The observed local mean times of contacts were :—

1st......	11 hrs.	50 min.	43·0 secs.
2nd	13 „	21 „	3·0 „
3rd	13 „	22 „	58·0 „
4th	14 „	43 „	54·5 „

The chronometer employed was rated by theodolite observations, and was probably correct within 1 sec.

Temperature Observations.—The result of the observations made for the two hours about totality were as follows :—

L.M.T.	In sun.		In shade.		
	Black bulb.	Glass bulb.	Dry bulb.	Wet bulb.	
h. m.					
12 21	99	93	90	73	
12 36	94	93	87	70	
12 51	90	90	85	69	
13 6	84	84	83	67	
13 21	79	78	80	66	Commencement of totality.
13 23	77	77	79	66	End of totality.
13 31	75	74	78	66	Lowest readings.
13 46	81	82	78	67	
14 1	89	86	82	67	
14 16	93	90	85	68	
14 31	97	94	86	69	

Magnetometer.—Colonel Gore made observations with the magnetometer with a view of detecting variation in the horizontal component of the earth's magnetic field during the eclipse. No change was observed.

Shadow Bands.—An attempt was made to photograph these with a small camera provided with an excellent Cooke lens of large aperture (F/6·3). A white sheet was stretched opposite to the sun's position, and a series of exposures was made at beginning and end of totality. Several spectators saw shadow bands, but no trace is discoverable on the photographs.

III.—*The Double-tube Camera.*

By Capt. Hills.

Instrument.—This camera was the one used by Mr. Taylor in Brazil in 1893, and was taken to Norway by Dr. Common in 1896. The tube is of wood, 6 feet long, and 14 × 7 inches in section, divided

by a partition into two square tubes of 7 × 7-inch section. In one of these was placed the "Abney" lens of 4 inches aperture and 5 feet 2 inches focal length, giving an image of the sun 0·57 inch in diameter; in the other the photoheliograph objective (used in Transit of Venus expedition), of 4 inches aperture and 5 feet focal length, with a Dallmeyer secondary magnifier of 7½ inches focus placed 5 inches within the focus, the combination giving an image of the sun 1⅜ inches in diameter. The camera was furnished with six plate-holders, each taking two plates of 160 × 160 mm., as in use for the astrographic chart, both plates being exposed by a quarter-turn of one shutter. The camera was pointed to a 16-inch plane mirror, made by Dr. Common, and mounted as a cœlostat by Mr. Hammersley after a design by Dr. Common, the sun's rays being thus reflected into the telescope.

The camera and cœlostat were not placed in a hut, but a screen of bamboo matting was erected round the whole instrument, to protect it from the wind, to which the cœlostat is particularly sensitive. Another portion of bamboo screen was placed horizontally above the camera, to protect the observer and the wooden body of the camera from the direct rays of the sun.

Mounting and Adjustment.—The cœlostat was placed on a masonry pier level with the ground. As some trouble had previously been experienced with the driving clock, owing to the heavy weight necessary, care was taken on this occasion that it should be very rigidly fixed in position. The method adopted was to screw the clock down on to a stout wooden base-board, which in its turn was firmly bolted to the masonry pier carrying the cœlostat, the driving cord being led off horizontally under one pulley attached to the base-board, and over another pulley hanging from the top of a strong wooden trestle about 6 feet high. Railway fishplates were used as weights. With this method no trouble at all was experienced, and the clock-driving was irreproachable.

In order to carry the camera, two parallel brick walls were built on the west side of the cœlostat, and on the top of each of these a 4-feet length of heavy rail was placed, held in ordinary railway chairs, lent for the purpose by the railway authorities at Pulgaon. A wooden stop or button fixed on the under side of the camera rested against the lower rail and prevented the camera from slipping down towards the mirror.

The angle at which the camera was set was so selected that the slide end should be at a suitable height for working. It was found convenient to direct the camera towards a point about 30° below the horizon, a little to the south of east. The focussing was done by reflection, and calls for no special remark, the final adjustment being accomplished by using the cœlostat mirror.

The adjustment of the axis of the cœlostat was effected very quickly by means of the attached declination theodolite. The level attached to the telescope makes it possible to adjust in altitude without any astronomical observation, for the latitude of the place can be taken from the map with sufficient accuracy; and setting the telescope to the south declination equal to the co-latitude, and in the meridian, the level should indicate horizontality. Index errors of the circle and level are eliminated by reversal of the instrument. There is a slight uncertainty attending the placing of the telescope in the meridian, but this does not seriously affect the adjustment in altitude. If a cross-level were made for the pivots of the telescope, this uncertainty could be removed.

To adjust in azimuth we must have an observation of the sun (or a star) at a distance from the meridian. Observing his declination (in reversed positions of the instrument and taking the mean), the instrument must be moved in azimuth until this observed declination agrees with that given in the 'Nautical Almanac.' A very few trials, if the sun can be seen for half an hour, will soon indicate the true azimuth without any calculations, within a minute or two of arc, though if the instrument be moved much in azimuth the altitude observation must be repeated.

After the initial adjustment of the cœlostat it was not disturbed, but the adjustment was re-tested at intervals, with the following results. The individual readings were only taken to minutes of arc, but both limbs of the sun were observed in both positions of the instrument, and the mean of the four set down:—

Cœlostat. Observer H.

Date.	Hour angle of sun. hrs.	Observed decl.	Tabular decl.	O.—C.
Jan. 15........	−3·0	21° 7′	21° 7′	0′
16........	−3·0	20 55	20 56	−1
17........	+2·0	20 40	20 42	−2
19........	−2·5	20 18	20 20	−2
20........	+3·0	20 6	20 7	−1

From this general watch kept on the instrument it is clear that the adjustments remained good within 1′ or 2′, which is more than sufficient for the purpose. The only other adjustment required is the setting of the face of the mirror parallel to the polar axis of the instrument. This was easily effected by reversing the mirror in the Ys and observing if the sun's image in the two cases crossed the same position on the ground glass when the mirror was moved in right ascension.

Three screws are provided in the base of the mirror cell for correcting this error should any be found.

Programme of Observations.—The six slides were filled and intended to be exposed as follows :—

No. of slide.	Exposure.	Plate.
1	2 secs.	Ilford "Empress."
2	8 "	Ditto, ditto.
3	12 "	Ditto, Special Rapid.
4	24 "	Ditto, ditto.
5	12 "	Ditto, "Empress."
6	4 "	Ditto, ditto.

Plates 2, 3, 4, 5 were backed with a solution of asphalte in benzol and had the Abney standard squares impressed on them. If the rapidity of the Special Rapid be taken as twice that of the Empress plates, the above programme gives a series of equivalent exposures of

2, 4, 8, 12, 24, 48 seconds.

No exposures of less than 2 seconds were made, because it was considered that the detail of the inner corona would be better shown by the large scale pictures, of which at least four separate sets were being taken by independent observers at other stations.

The orientation of the corona was determined by the following method: After the last exposure had been completed the clock was stopped and the whole instrument was left untouched, with the slide still in the camera, till night. The Abney lens was then uncovered for about 2¾ hours, the mirror being left stationary, and a series of star trails were thereby drawn across the plate.

A recorder was employed whose duty was to note the exact time of each exposure made during the eclipse. They were as follows :—

Slide No.	Exposure according to programme.	Actual exposure.				
		Shutter opened.		Shutter closed.		Duration.
		Beginning of totality = zero.				
	secs.	min.	sec.	min.	sec.	sec.
1	2	0	5·0	0	7·5	2·5
2	8	0	14·0	0	22·0	8·0
3	12	0	27·5	0	39·0	11·5
4	24	0	47·0	1	11·0	24·0
5	12	1	18·5	1	30·5	12·0
6	4	1	37·0	1	41·0	4·0

All the above plates were successfully developed the night after the eclipse, and positive copies on glass were made to guard against loss.

A reproduction of one of the best photographs (No. 3, Dallmeyer lens, 12 secs. exposure), is given in Plate 1 (frontispiece).

IV.—*The Spectroscopic Cameras.*

By Capt. Hills.

Instruments.—The details of the two spectroscopes used were as follows:—

	Spectroscope No. 1.	Spectroscope No. 2.
Objective	Cooke achromatic, 4½ in. aperture, 5 ft. 10 in. focus.	Single quartz lens, 5 in. aperture, 4 ft. 9 in. focus.
Collimator and camera lenses.	Single quartz lens, 2½ in. aperture, 30 in. focus.	Single quartz lens, 3 in. aperture, 36 in. focus.
Slit	1½ in. by 0·0018 in.	2 in. by 0·0014 in.
Prisms	Two dense flint prisms of 60°, 4½ in. base, 2½ in. height.	Four double quartz prisms of 60° (each prism being composed of two half-prisms of right- and left-handed quartz), 3½ in. base, 2½ in. height.
Prisms at min. deviation for	H_γ.	H_ϵ.
Position of slit with respect to sun's image.	Parallel to meridian through sun's centre, cutting limb at point of second contact.	Vertically diametral.

The slits were in each case adjusted to such a width as to realise one-seventh of the theoretical maximum resolving power of the prisms. In the case of spectroscope No. 1 which, as will be shortly seen, was used for most of the work, this amounted to about 0·3 of an Angström unit in the violet. It may be noted that any higher degree of resolving power would have been wasted owing to the coarseness of grain of the photographic plate, the above figure representing not only the calculated resolving power of the instrument, but that actually realised on a trial plate.

The length of the spectrum on the plate was 3¼ inches from H_β to K.

Both spectroscopes were mounted in an approximately horizontal position, and were supplied with light by a heliostat, furnished with a 12-inch flat mirror.

Erection and adjustment of Instrument.—The heliostat and spectroscopes were placed on masonry piers, and a hut of bamboo matting was built up round the latter. The heliostat was left in the open, and, such was the dryness of the air, that it was found that a sheet tied over it at night was more than sufficient to protect it from any damp.

The adjustment of the polar axis of the heliostat was carried out by means of an attached theodolite in precisely the same manner as has been described above in the case of the cœlostat. As it was not possible to reverse the instrument when the slow motion in right ascension was attached, the position of the axis when once adjusted was not retested. This, however, was of little importance, as great accuracy in the driving is not required for this work.

The adjustments of the spectroscopes call for no special mention.

Programme of Exposures.—Two separate lines of work were undertaken :—

(1) The recording of the spectrum of the corona—using for this purpose both spectroscopes, and giving only one exposure of as long a duration as possible.

(2) The recording of the "flash" or spectrum of the sun's limb at both the beginning and end of totality.

For this purpose, spectroscope No. 1 only was used, the camera being provided with a sliding plate, by which means a large number of successive exposures could be made at short intervals.

It was intended to begin the exposures about 10 seconds before second contact, and to continue them till 7 seconds after it, and to expose a similar series at third contact.

In order to get the latter series, it was necessary to shift the image on the slit, which was done by the slow motion of the heliostat, an assistant being stationed at the latter, watching the sun through the theodolite telescope attached to the polar axis.

All the available time during totality, was employed in the long exposure for the corona spectrum.

The complete programme of exposures as drawn up, was as follows, the expected duration of totality being 115 seconds :—

Spectroscope.	No. of slide.	Exposures.	Time in totality.	Plate.
No. 1...	1	10 of 1 sec.	−10 to +7 secs.	Lumière "green sensitive."
	2	1 of 85 secs.	15 to 100 secs.	" " "
	3	10 of 1 sec.	108 to 125 secs.	" " "
No. 2...	*1*	*1 of 98 secs.*	7 to 105 secs.	Ilford Special Rapid.

All the essential points of this programme were carried out The actual time of each exposure was as accurately as can be ascertained,

2nd contact: −10, −8, −6, −4, −2, 0, +2, +4, +6, +8, seconds.
3rd „ −3, −1, +1, +3, +5, +8, +10, seconds.
Corona: Spectroscope No. 1. Exposed 79 seconds from 21—100.
„ „ No. 2. „ 98 „ „ 7—105.

The second series did not begin quite as soon as had been intended, and between the fifth and sixth exposures there was rather a longer interval than between the others, owing to a slight mistake on the part of the exposer. This is, however, of no consequence, as all the interest centres about the photographs taken within 5 seconds of the contact.

The Corona Spectrum.—A reproduction of the corona spectrum as obtained by the two-prism flint spectroscope is given in Plate 2. It is obvious that the photographic intensity of the continuous spectrum fell off very rapidly on receding from the limb. No trace of it is seen at a greater distance than 4′.

Five strong bright lines of unquestionably coronal origin are to be seen. Their wave-lengths, and relative intensities may be provisionally given as—

λ (Rowland).	Photographic intensity.
3987·0	5
4233·5	10
4360·0	3
4567·9	8
5316·9	8

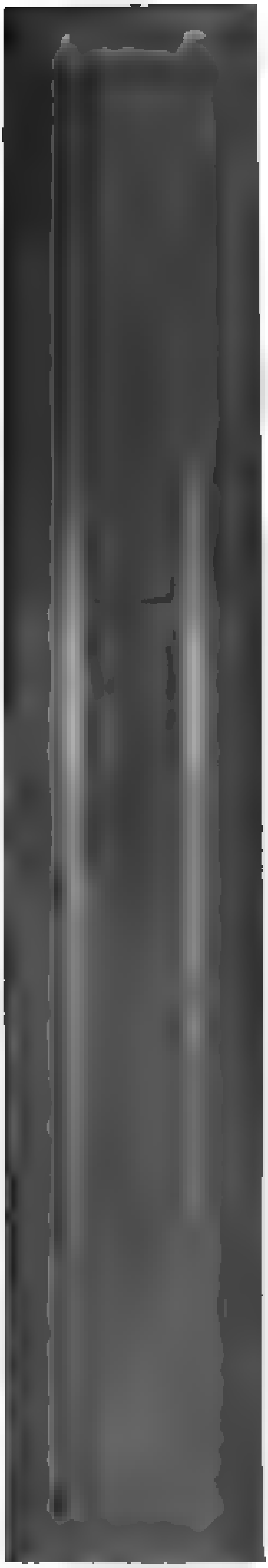
There are other fainter lines whose wave-lengths have not yet been determined.

Spectroscope No. 2, with the quartz train, gave a corona spectrum stretching a considerable distance into the ultra-violet, but of feeble intensity. A strong bright line occurs at λ 3801·0, and the lines given above are also to be plainly seen with the exception of the well known line in the green which was outside the plate.

Spectrum of the limb.—The two series of spectra of the limb contain an immense amount of detail, and will take a considerable time for complete examination. As an indication of the character of the results obtained, reproductions of portions of the two spectra taken at second and third contacts are given in Plate 3.

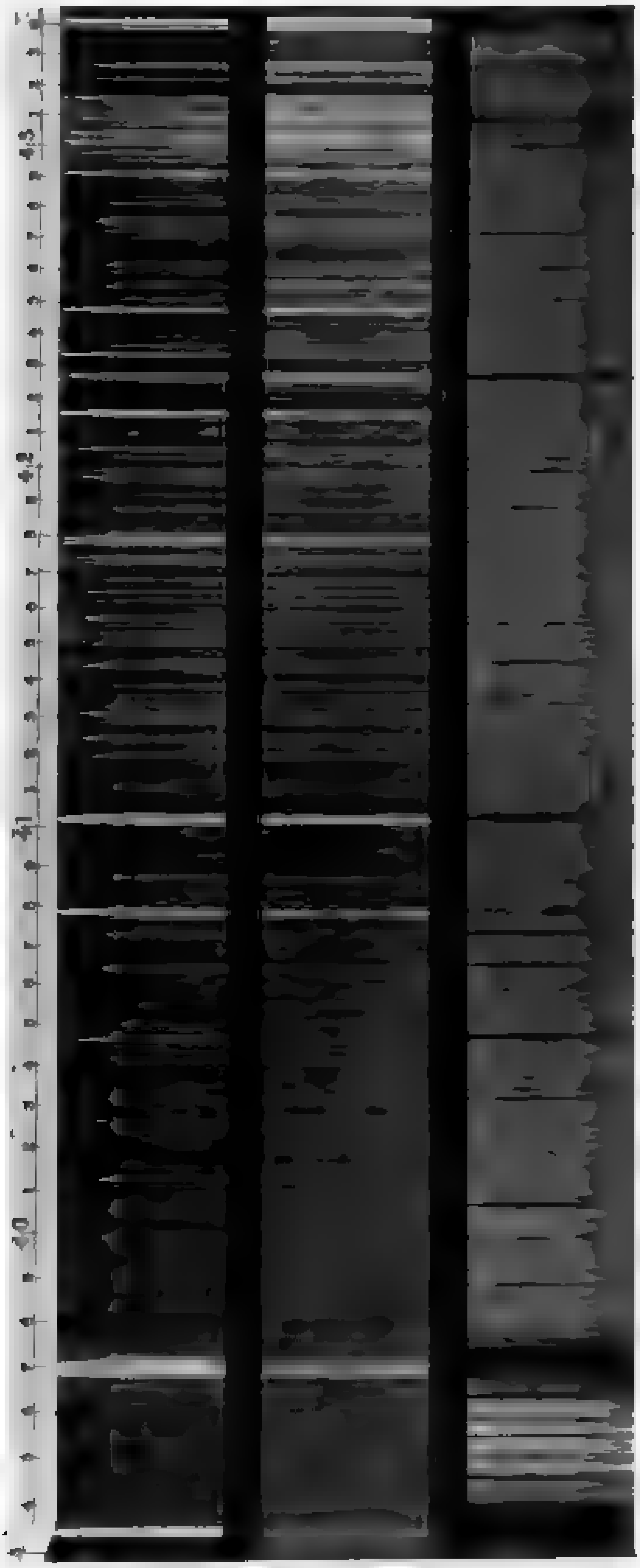
As considerable interest attaches to the question of the connection between the bright line spectrum of the limb and the solar spectrum, a reproduction of the latter, taken with the same instrument is appended.

CORONA SPECTRUM.



From photograph taken with a 2-prism flint spectroscope, at Pulgaon, by Captain E. H. Hills, R.E. Enlarged 1½ times.

SPECTRUM OF SUN'S LIMB.
(Region H, to K.)



From photographs taken with a 2-prism flint spectroscope, at Pulgaon, by Captain E. H. Hills, R.E. Enlarged 3½ times.

V.—*The Spectroscope with Two Slits.*

By H. F. Newall.

It was intended to attempt (i) to determine by a spectrographic method, the difference in velocity in the line of sight in the eastern and western equatorial regions of the corona, (ii) to utilize the same material, as was obtained for the first research, for a comparison of the spectra of widely separated parts of the corona, and (iii) to use the same instrument in securing photographs of the bright line spectrum of the sun's limb at the end of totality, to be used for the accurate determination of the wave-lengths of the bright lines.

The instrument provided for these purposes is a four-prism spectroscope with two slits.

The train of prisms is of such dimensions and construction, as to transmit a 2-inch beam of light, and to produce a minimum deviation of 180° for H_γ. The collimator and camera are set parallel to one another.

The whole spectroscope is mounted so as to turn about an axis, parallel to the collimator. The axis is rotated (with a period of 24 hours) by clockwork, and is tilted so as to be parallel to the earth's axis. In this position the collimator points to the north pole.

The tube of the collimator is prolonged beyond the plane of the slit, and is arranged to carry at its end a mirror of speculum metal and an object glass, by means of which an image of the sun can be thrown upon the slit.

The whole arrangement thus consists of a spectroscope combined with a polar heliostat, and in virtue of the fact that the spectroscope is rotated together with the mirror, the image of any celestial object thrown upon the slit does not rotate relatively to the slit. Furthermore, the mirror is mounted in such a manner that the axis about which it can be tilted—namely the declination axis—can be oriented relatively to the collimator tube, so that any diameter of the sun may be set parallel to the slit.

The two slits with which the collimator is provided are parallel to one another in the focal plane of the collimator lens, and are separated by such a distance that when the image of the eclipsed sun is thrown between the slits, one is illuminated by the eastern, the other by the western equatorial region of the corona. The top half of one slit is covered, and also the bottom half of the other. The exposures for the two sides of the corona are made simultaneously, and the resulting photograph should give two spectra side by side on the same plate, one slightly displaced, relatively to the other, by an amount depending on the separation of the two slits and the construction and adjustments of the spectroscope.

It was decided that certain coronal lines in the neighbourhood of

λ 4233 would be the best available for the determination of displacement due to velocity.

The linear dispersion in the photographed spectrum is about 15 tenthmetres per millimetre at H_γ. The relation between the velocity in the line of sight and one complete revolution of the micrometer to be used in the measuring the plate is about 260 kilometres per second for one revolution.

The scale of the photograph is such that one degree on the sky corresponds to about 9 mm. on the plate.

The effective aperture of the combination regarded as an instrument for producing monochromatic images of a slit-shaped region of the corona is $\frac{1}{12}$.

The adjustment of the axis of the instrument to parallelism with the earth's axis, was accomplished in the same manner as that adopted for adjusting the cœlostat. A theodolite with declination circle was attached to a part of the frame of the spectroscope, specially prepared for it, between the camera and the collimator. The adjustment was very easily and satisfactorily made: in altitude by observations made with the spirit level attached to the theodolite-telescope, and in azimuth by observations of the sun made some hours before or after noon.

Programme of Exposures and General Results.

I. *Spectra of the Corona.*—A set of five photographs, from which the relative velocity in the line of sight of the eastern and western equatorial regions of the corona could be deduced, was to be taken with the spectroscope with two slits.

The programme of exposures was carried out completely successfully, as follows:—

Thirty minutes before totality, plate A was exposed for 15 seconds for spectra of sunlight diffused from the sky near the sun.

Fifteen minutes before totality, plate B was exposed for 20 seconds for a duplicate of plate A taken in falling temperature.

During totality, plate No. 1 was exposed for 100 seconds for the spectra of the eastern and western regions of the corona..

Fifteen minutes after totality, plate C was exposed for 15 seconds for spectra of sunlight diffused from the sky near the sun.

Thirty minutes after totality, plate D was exposed for 15 seconds for a duplicate of plate C under different temperature conditions.

Result.—On development, the photographic plate No. 1 showed no trace of any impress of the coronal spectra, though the development was pressed as far as possible.

It is clear that the failure is due to the faintness of the corona in *the region photographed.* Captain Hills was successful in photo-

graphing the spectrum of the corona at the same station, but the radial extension of the bright lines and also of the continuous spectrum is unexpectedly small. In neither of his photographs is the spectrum traceable further than about 4′ from the limb of the sun. N., basing his attempt on the results obtained by Deslandres in 1893 and by Abney and Thorpe, had tried to photograph the spectrum at nearly 8′ from the limb of the sun. The apparatus used on the present occasion was of such design and construction, that it was expected to give considerably brighter images than those used by Deslandres in 1893.

II. *Spectrum of the Sun's Limb.*—With the same spectroscope, photographs were to be taken of the bright line spectrum of the sun's limb at the end of totality.

Ten seconds before the end of totality, the exposure referred to in the preceding paragraph was completed; and whilst another plate-holder was being placed in the camera of the spectroscope, the adjustments in R.A. and Declination were changed so that the image of the chromosphere, which was being disclosed near the point of third contact, was adjusted on one of the two slits. Four exposures were then made in rapid succession.

Results.—The photographs thus obtained, give spectra ranging from about λ 3900 to λ 4900; and the first of the series contains a vast number of bright lines, generally similar to those seen in Captain Hills' photographs, and to those shown in the photograph obtained by Mr. Shackleton in Novaya Zemlya, 1896, August 9, and reproduced in Sir Norman Lockyer's Preliminary Report,* and also to those obtained by Mr. Fowler in 1893, and reproduced in Sir Norman Lockyer's Report.†

An additional point of interest in the first photograph of the series is that many absorption lines are also visible. A cursory comparison with the solar spectrum discloses the interesting fact that these lines differ in intensity from the absorption lines in the ordinary solar spectrum.

VI. *The Objective Grating Telescope.*

By H. F. Newall.

An objective grating telescope was used for visual observations of the coronal ring in the green light of wave-length 5316·9 (1474 K).

A plane grating, by Rowland, 14,438 lines to the inch on a ruled surface, $3\frac{1}{2} \times 2\frac{1}{8}$ inches, was fixed on a turn-table in front of a telescope of focal length 29 inches and aperture $3\frac{1}{2}$ inches. A positive eye-piece was used which gave a magnifying power of 19·2, and whose circular field of view was rather more than 1° in diameter.

* 'Phil. Trans.,' A, vol. 189 (1897), pp. 259—263.

† *Ibid.*, A, vol. 187 (1896), Plate 14.

The instrument was mounted so that the telescope was parallel to the earth's axis and pointed towards the north pole. The grating was used in a manner analogous to that in which the mirror of a polar heliostat is used. The light of the corona was incident on the grating at an angle of about 57°, and diffracted beam utilised in the telescope left the grating at an angle of about 13°. In this position of the grating, the green of the second order was used, and the magnifying power of the grating was a little greater than ½, so that the distorted coronal ring was an ellipse, in which the major axis was about twice as great as the minor axis; the minor axis was parallel to the length of the spectrum and perpendicular to the direction of daily motion. No clockwork was used, but a slow motion of a very simple construction was provided and found to work perfectly satisfactorily. The observations were begun 6 seconds after the beginning of totality, and were completed in about 70 seconds.

Results.—The coronal ring was seen in the spectrum of the second order with great distinctness and with such brilliancy as to leave no doubt that it could have been photographed.

None of the fine radial structure of the corona could be seen, though it was especially looked for; but broad patches of light were clearly visible in different positions round the ring.

A drawing of the brighter extensions was made during the eclipse, the observer (N.) keeping himself intentionally in ignorance of the orientation of the image seen in the eye-piece until after the observations were completed so as to avoid bias. A preliminary comparison of the drawing with the direct photographs of the corona has been made, and the following general statements will probably not require much revision on a closer comparison:—

(i) There appeared to be glowing "Coronium" (assuming that the radiation of wave-length 5316·9 is rightly attributed to an element "coronium") at all points round the sun's limb extending radially to distances estimated as ranging between 4′ and 14′.

(ii) The luminosity was not uniform round the limb, but in no position was it entirely absent.

(iii) No *fine* radial streamers comparable with those seen near the poles of the sun in ordinary direct photographs of the corona were observed, though this fine structure was specially looked for.

(iv) In certain positions round the limb patches of increased luminosity were seen; in all, seven patches were noted; in *several cases the* extension was considerably greater in a *radial direction* than in a tangential. The bases of the

broad streamers on the limb subtended angles ranging from 10° to 30° at the centre of the sun's disc, and the radial extension in three cases was estimated as being greater than 12′.

(v) Two of the long streamers referred to in the last paragraph were found to coincide roughly in position with marked broad extensions in the direct photographs of the corona, viz., that to the N.E. and that to the S.W. But the third long streamer to the N. seems to have no connection with any obvious extension in the photographs.

(vi) There was no marked "coronium" luminosity corresponding either to the double-rayed extension in the N.W. quadrant or to the broad extension in the S.E. quadrant.

(vii) As far as it has been possible to pursue the investigation at present, there has appeared no relation between the position of the brighter patches of coronium and the prominences, except perhaps near the three prominences in the N.W. quadrant.

VII. *Polariscopic Observations.*

By H. F. Newall.

It was intended to devote any time that remained over, after providing for the three foregoing investigations during the eclipse, to (i) a search for faint extensions of the corona with the aid of a polariscope, or as an alternative (ii) a general investigation of the nature of the polarisation-phenomena visible during an eclipse. It was expected that results obtained in the latter investigation would probably only be serviceable in suggesting methods of research for future eclipses.

The polariscope used consists of a Nicol prism with a Savart plate attached in front of it. The field of view of the instrument is lozenge-shaped after the manner of Nicol prisms, the long axis being 29° long and the short axis 24°. The width of the central band due to the Savart plate was approximately 1° 25′ between the centres of the first lateral dark bands, the centre being regarded as that part of the dark band where the dusky red meets the steel blue. The plate had been adjusted relatively to the Nicol prism, so that the bands when visible were parallel to the principal plane of the Nicol, and they were kept in this relative position throughout the observations. The instrument was used without telescope or circles.

Observations.—When first the instrument was put to the eye, about 85 secs. after the beginning of totality, bands were visible over the whole field of sky seen through the Nicol prism. Not only were the alternations of brightness seen, but the colours of the bands appeared

with unexpected vividness. They were seen at all points within 30° of the sun, with little or no variation in vividness, and as the instrument happened first to be held, the bands were approximately parallel to the sun's axis. These vivid bands are attributed to the polarisation of the light scattered (diffracted) by solid particles in the earth's atmosphere.

The bands were so disconcertingly vivid that a few moments were wasted in inspection of the instrument, but immediately afterwards observations were quietly renewed, the search for faint coronal extensions was abandoned, and attention was confined to the phenomena of polarisation.

The instrument was rotated about its axis, and the bands faded from view and became invisible in a certain position. It was thought immediately after the eclipse that the observations made were enough to prove that the plane of polarisation was neither vertical nor horizontal, but it has since been found that the evidence is not such as to warrant this statement. When the bands had become invisible in the outer part of the field of view, the rotation of the instrument was discontinued for a moment. The eye then gradually became aware of faint colours over the corona, but the distribution appeared to be uneven—rather in patches than in bands. These colours are attributed to the polarisation proper of the corona. The "patchy" distribution is doubtless a result of the nature (presumably radial) of the polarisation of the corona, and of the largeness of the scale of the bands compared with the diameter of the moon (1° 25′ : 33′). The fact that the colours appeared faint in contrast with the vivid sky-bands previously seen may be referred to several alternative explanations which cannot well be dealt with here in detail. It is obvious that the ratio of the brightness of the light scattered by the sky to that of the light of the corona plays as important a part as the proportion of the coronal light that may be regarded as polarised.

Next, attention was directed to the corona near the limb, and the central part of the central band was observed while the instrument was rotated, the central band being kept radial to the sun's disc. The observed central part was seen to be bright at all points round the limb on the east side, whether the central sky-band were bright or dark.

Then the bands were set so that one of the first dark lateral bands was tolerably close to the moon's limb, with its centre perhaps half a moon's diameter from the limb. The band was observed to be bright near the limb, and to be dark at a short distance on either side.

Both of the last mentioned observations point to the idea that the *light scattered by the* atmosphere was comparable in brightness with *the corona at points* not far distant from the sun's limb.

The incompleteness of the observations is recognised, but on the whole it would appear that their suggestiveness justified the observer in devoting 15 seconds to making them.

The observed intensity of the bands, attributed to sky polarisation, is evidence of the quantity of light reflected by solid particles in the atmosphere. It seems not improbable that the unexpected brightness of the general sky and landscape during totality may have been connected with the amount of light reflected from the dust suspended in the atmosphere and illuminated by the sun-lit plains outside the moon's shadow. The light colouring of the plains, due to the dried herbage at that time of year, is very marked at Pulgaon, but it must not be forgotten that the "sun-lit plains" were in the moon's penumbral shadow for more than an hour both before and after totality.

The observations occupied the 15 seconds, ending 15 seconds before the end of totality.

About 30 seconds after the end of totality, polarisation was again looked for, but no trace could be detected near the sun in any position of the instrument.

"The Skeleton and Classification of Calcareous Sponges." By G. P. Bidder. Communicated by Adam Sedgwick, F.R.S.

Received May 6,—Read May 26, 1898.

I. *Skeleton.*

An element which seems to have been too little regarded in the physiology of sponges is the permanent tension of their walls.* The contours of the surfaces, particularly where they rise over projecting spicules, are alone sufficient to demonstrate that there is surface-tension between the protoplasm of the sponge and the sea-water. Both the outer and the inner surfaces of a cylindrical or of a spherical sponge unite, therefore, in exerting a force which tends to contract its diameter. In many sponges there would appear to be also some form of elastic matter in the tissue immediately underlying the collar-cells; since in teased preparations of the living sponge, fragments of the chamber-wall turn inside out and swim about like ciliate larvæ. While the collar-cells are active, these united tensions are resisted by the pressure of the water in the cavities they line. A broad generalisation of the mechanism of a sponge's currents shows that the velocity in the oscular stream (of comparatively narrow

* I have to thank Mr. G. T. Walker, of Trinity College, Cambridge, for rescuing me from some fallacies with regard to the effects of this tension, and Professor Lewis for most kind patience in mitigating my ignorance of crystallography, and much valuable information.

sectional area) is the manifestation of a pressure in the flagellate chambers (of broad aggregate sectional area); which pressure is maintained by the moving flagella, and resisted by a normal tension in the walls of the chambers or of the sponge.*

I therefore regard the contraction recorded by Minchin in *C. clathrus* (and undoubtedly occurring in unfavourable conditions, not only in this, but in many other sponges) as the passive result of permanent tensions in the sponge-walls, which are no longer counteracted by the comparatively inactive flagella.† It is with the advantage of preventing such contraction being a consequence of inactivity that a skeleton has been evolved in most sponges: whatever the substance of which it is composed, this skeleton tends to take in surfaces a triradiate [more rarely sexradiate] arrangement.

[If a uniform elastic membrane be extended by a star of rigid rods, the area extended is greatest in proportion to the material involved, when the star is equiangular and has either four or five rays; though there is comparatively little advantage for any number from three to six. Where, however, the rigid rods unite in a network, the triradiate (*i.e.*, hexagonal) arrangement is the most economical possible, and the triangular (or sexradiate) the most rigid. The square superficial network is less unstable but less economical than the hexagonal; it would seem rarely to occur except where—as in the Hexactinellida and in *Reniera*—a definite cubic mesh has been developed to support a frothy tissue of three dimensions (*cf.* Schulze (11)).—*July* 23, 1898.]

Minchin, after a most interesting account of the ontogeny of triradiate and quadriradiate spicules‡ in reticulate Ascons, writes (21, p. 549):—"Crystallisation cannot now be taken as an adequate explanation of the external form of the spicules . . . It is . . . clearly impossible that the triradiate system§ should owe its form,

* *Cf.* (18), p. 29. I still hope to write more in detail on this subject.

† The flask-like form of the ectocytes I still hold to have been "developed to expose the greatest possible surface to the medium from which the excreted" (or secreted) "substance is derived," (16), p. 480. The evidence appears to me still in favour of these cells being excretory; but whatever their secretion, it is important to the sponge, and did they not take this form on contraction of the sponge, their surface in contact with the parenchymal jelly would be disadvantageously reduced. I regard as an extreme case of this the spongoblasts [considering the fibre to be an invaginated tube of cuticle contracted by its own elasticity until it has become a solid rod.]

‡ I may mention that *Sycon compressum* has hair-spicules about $\frac{1}{10}\mu$ in thickness which are each formed by two cells, as Minchin describes for a ray of a triradiate in *Clathrina*; and the young giant club-spicules also show two formative cells. For such pairs of cells I suggest the name "adelphidia"; they were described by Lendenfeld (8) and Stewart (10) as sense-cells.

§ The "triradiate system" is a word used by Minchin to denominate the three rays of the triradiate spicule whether or not an adventitious fourth ray be added

as a whole, to the action of crystallisation." If I understand my friend correctly, I differ from him essentially. I regard the spicules of calcareous sponges as being skeleton crystals of calcite. The direction of the optic axis is fixed (probably by the conditions of pressure and tension prevailing at the moment) when the spicule first makes its appearance. The formed material from which the calcite crystallises is commonly limited by biological conditions to a narrow interval between two similar surfaces. These surfaces may have the form of a comparatively wide cylinder, as in the case of *Clathrina clathrus* or *Leucosolenia Lieberkühnii*, a narrow cylinder, as in the case of the stalk of *Guancha blanca*, or a more complex irregular form, as in the afferent or radial canals of the Heterocœla. I suggest that the triradiate spicule is in all cases the skeleton of that region of a hexagonal prism, with the given optic axis, which is enclosed by the two surfaces which limit the formation of material.

It is impossible to profess, in a limited space, or indeed with the limits of our available knowledge, to give anything approaching a complete harmony of all known forms of spicules with the foregoing hypothesis. Treating of the simplest, the regular triradiates of *Clathrina clathrus* and *Guancha coriacea*, I have been led to the above conclusion primarily by observation of deformed spicules; of which I sketched all examples met with during many months examination of fresh preparations at Naples, and for which I have

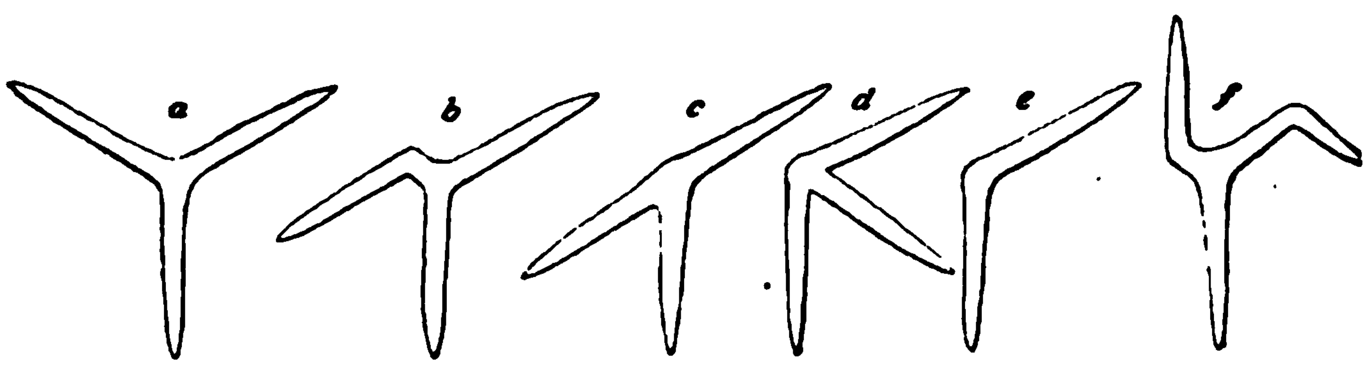


FIG. 1.—Spicules of *Guancha* (*Leucosolenia*) *coriacea* (Mont.) from Naples, × 120. *a* is normal, some hundreds of this type occurring for one of all the others. In this species and *C. clathrus* 26 or 27 deformed spicules were recorded. 15 belonging to type *b*, 6 to type *c*, 3 to type *d*, 1 to type *e*, and 1 to type *f*. In addition to these, a drawing was lost of a spicule of type *b*, but with the ray bent a second time to its original course, and Ebner gives a drawing of a plane quadriradiate X-shaped spicule (in *C. clathrus*), the four angles being alternately 120° and 60°. No other deformed spicule was observed in these species. [Ebner also draws a form resembling *f*, but with the two flexions symmetrical and in opposite senses.]

since carefully searched my permanent preparations. Drawings of all types met with, among some thirty examples, are given in fig. 1. It will be seen that they are all expressed in one statement,

perpendicular to them. I shall use the term "triradiate" where necessary with the same meaning.

that part or the whole of one ray deviates at an angle of some multiple of 60° from its normal course. That all deformities in these species should be reducible to this law appears to me inexplicable on the supposition that they are due to any biological variation of the formative cells; and it will be observed that the variation in which two of the angles between the rays are of 60° is wholly irreconcilable with the "honeycomb" theory of contiguous circles appealed to by Minchin. Ebner (12) has worked out the relations of many forms of spicule to the optic axis and the hexagonal prism: he sums up against regarding the spicules as mere crystals. I can only claim to have a little extended his observations, but that little is all in favour of exact obedience to crystalline laws.

Nitrate of potash has a crystalline form resembling almost to identity that of calcite. Fig. 2 is copied from Lehmann (13),

FIG. 2.—Nitrate of potash crystallising in caustic potash. (Copied from Lehmann (13), fig. 99.)

and shows the form taken by nitrate of potash crystallising in the presence of caustic potash. Fig. 3, placed near it for comparison,

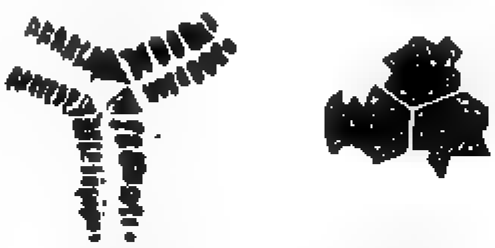


FIG. 3.—Last remains of spicules (× 1000) of *Guancha coriacea*, decalcified slowly in glycerin.

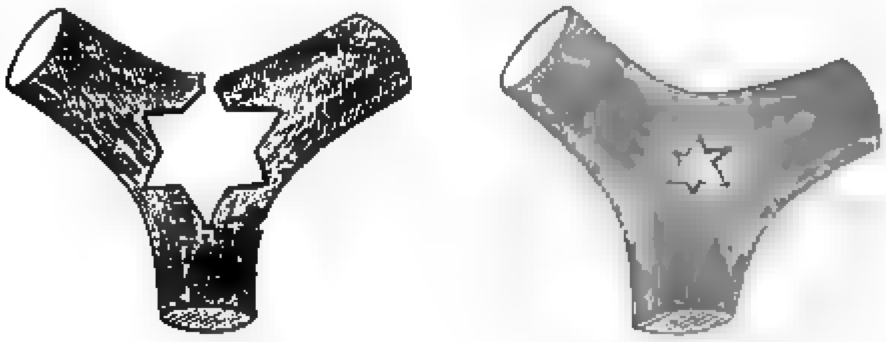


FIG. 4.—Spicules of *Clathrina clathrus* (× 1200) partly decalcified in glycerin. *The sharp truncation is conventional, to show that only part of the rays is drawn.*

shows the last rudiments of the spicule of *G. coriacea* slowly decalcified by the prolonged action of glycerin. Fig. 5 gives decal-

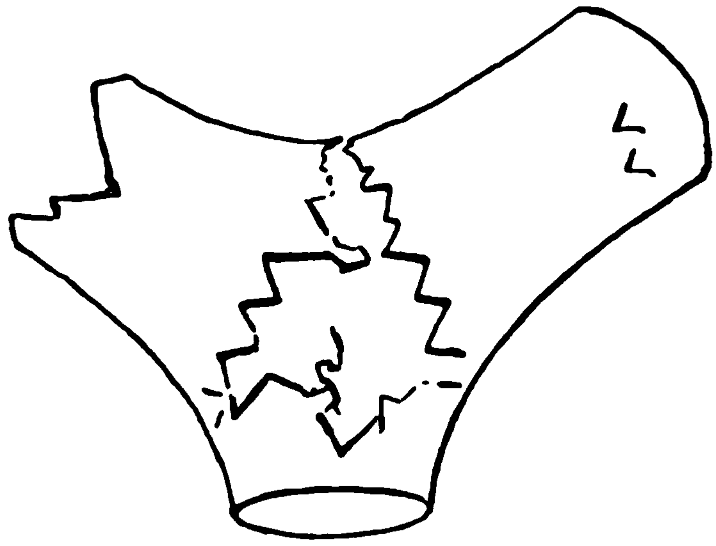


FIG. 5.—Spicule of *G. coriacea* (× 1500), showing sculpturing by decalcification in Canada balsam. One ray is fractured and decalcified, the other two conventionally truncated.

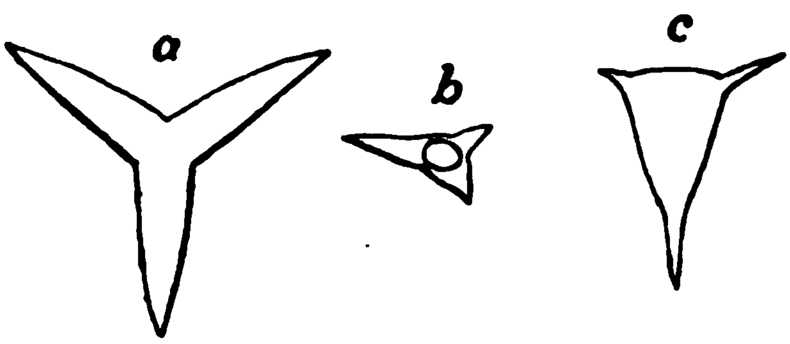


FIG. 6.—Young spicules (× 1000). *a* of *C. clathrus*, *b* (an equiangular spicule in perspective) of *A. cerebrum*, *c* of *L. Lieberkühnii*.

cification figures on a spicule belonging to the same sponge under the action of Canada balsam, and fig. 4, spicules of *C. clathrus* attacked by glycerin. They appear to be generalisable, like those drawn and explained by Ebner for *Leucaltis solida*, into triangles whose angles are on the external face of the spicule opposite the rays, and intermediate to them on the gastral face; complementary, as are the edges on the opposing poles of a rhombohedron: the crystallisation is therefore closely comparable with that of Schiefer spar (a natural form of calcite crystallising in flat hexagonal plates), and still more with that of a specimen of dolomite (calcite with a little magnesium), No. 427 in the Cambridge museum, which is crystallised in flat triangular plates. The rays of the spicule are opposite the faces of the rhombohedron on the gastral surface, opposite the angles of the rhombohedron on what may be called the "dorsal" surface.*

* [The initial etchings by Canada balsam on the dorsal surface of the rays of *G. coriacea* frequently appear under the 2 mm. immersion lens to be small rhomboids, orientated so that lines bisecting their acute angles are parallel with the morphological axis of the ray. In some spicules of *C. clathrus* prolonged decalcification by glycerin yielded internal cavities of similar outline. The optic axis,

The fourth or gastral ray of a quadriradiate spicule is an acute rhombohedron which crystallises about the principal axis when the cellular conditions allow of material being deposited there, consequent (following Minchin's account) on the adventitious occurrence of another calcogenous cell on the gastral surface. That this crystallographic interpretation is correct is indicated by the frequently triangular section (fig. 7) of the fourth ray, the faces of the triangle

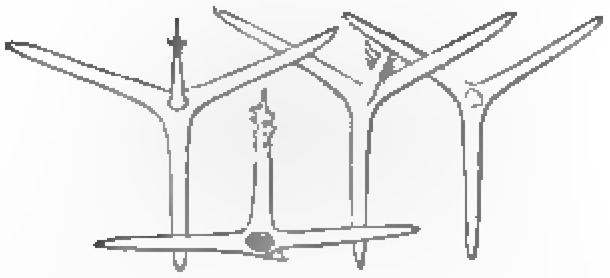


FIG. 7.—Three-spined quadriradiate spicules of *A. cerebrum* in perspective; the interior of the distant rays are in one spicule indicated by shading. The fourth spicule is drawn from above; it is not spined, but the apical ray shows a distinctly triangular section. All × 260.

being transverse to the rays of the triradiate, while a strong confirmation of the whole view is afforded by the thorns on the fourth ray in *Ascaltis cerebrum* (fig. 7). These, when viewed from above, are always seen to lie accurately over the rays of the triradiate,* either as definite minute triradiate spicules, or as three rows of thorns; and therefore crystallise on the three faces of the rhombohedron. The whole quadriradiate, with fourth ray and thorns, extinguishes simultaneously in four positions between the crossed nicols. I have verified on *G. coriacea* and *blanca* [and *A. reticulum*] Ebner's observation, that the optic axis of these equiangular spicules is perpendicular to their plane.

I suggest that the ancestors of these sponges had a skeleton consisting of organic rods, partially impregnated with carbonate of lime; that these rods united in a triradiate grouping and that—whether for cementation or merely for strength—the calcareous secretion increased to the point of crystallisation. The original triradiate form was due to an instinct, apparently acquired by the skeletogenous

with uniaxial optic picture, being vertical to the plane of the spicule, interpretation of these figures is not obvious.

Glycerin commonly first decalcifies the axial parts, presumably because the liquid enters most freely where there is greatest admixture of organic matter. Canada balsam (except where the sponge has been killed directly in chloroform) first attacks the surface, presumably because the more permeable regions are protected by consolidated balsam.—*July* 23, 1898.]

* I have once or twice recorded a twist of 5° or 10°; the thorns still being inclined at 120° to each other. This, if established, would form another resemblance with the crystalline forms of dolomite.

cells of all sponges, of placing themselves in the direction where the alternate thrust and tension, caused by expansion and contraction of the sponge, is maximal.* The carbonate of lime crystallised as calcite, in three-rayed stars, the optic axis being radial to the sponge, in the line of greatest pressure,—and Professor Lewis has shown me an example of calcite from Freiberg where the rhombohedra are etched and infiltrated in three-rayed stars, whose rays bisect the rhombohedral surfaces (fig. 8). These crystalline triradiates being them-

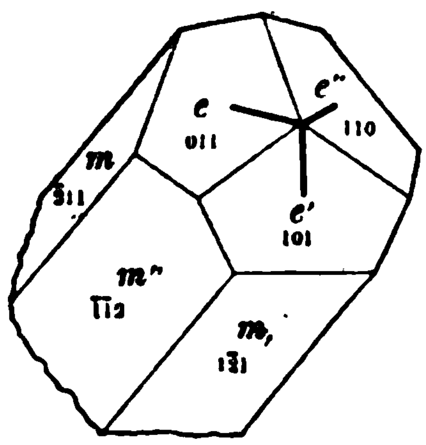


FIG. 8 (from a drawing by Professor Lewis).—Crystal of calcite infiltrated by opaque black matter (probably a metallic oxide), which forms a three-rayed star, shown by the strong short lines in the diagram. The rays bisect the angles made by the edges of the faces *e*, *e'*, *e''*, and therefore lie in the planes of symmetry of the crystal. The black matter is in part underneath the faces, so that the reflecting surface can be seen to be uninterrupted over it. The specimen in the Cambridge Collection contains upwards of 120 crystals in which the star is conspicuously seen.

selves of the form advantageous for skeletal elements, the formative cells lost their directive instinct, and are now passively carried on the point of the growing crystal, showing no trace of individual character except in mere stalactitic modification of the crystalline contours. In the ancestors of *Leucosolenia*, *Sycon*, and *Leucandra*, crystallisation appears to have taken place while as yet only one cell (following Minchin) was concerned with the formation of the calcareous element. As in the reticulate Ascons, the optic axis lay in the line of greatest pressure, that is, vertical to the surface of the flagellate gastral cavity; but in the former case the principle of

* [The first combination of the primitively isolated spicules would be to form longitudinal spicule-fibres; since, on account of the open osculum, the surface-tension parallel to the axis of the cloaca is only in a slight degree opposed by internal pressure. To oppose the transverse strain, which would follow on variation in flagellar activity, such fibres must be united; this I conceive to have been effected in the simplest manner by two spicules, instead of one, applying themselves to the end of a fibre, and diverging to meet the circumferential stress. Thus would be produced longitudinal reticulations of branching spicule-fibre, resembling the skeleton which appears in many thin-walled Monaxonida; on this conception the triradiate represents phylogenetically the fork of a pair of branches.—July 23, 1898.]

the existing triangular grouping of the formative cells determined the crystallisation of the calcite in a flattened (pinacoid) form, extended transversely to the optic axis, as in Schiefer spar: the unicellular crystals appear as [elongated] rhombohedra [*cf.* Sollas (9) p. 389] which form the primitive acerates. The ontogenetic history of the triradiates of *Leucosolenia* is not yet known. It is possible that the rudiments originally form an acute angle with the morphological axis of the sponge, and that the formative cells repeat a change which has taken place in evolution, with the advantage of the production of a [gastral from an ectocytal] skeleton. The causes governing the direction of the optic axis in this and higher sponges are still not clear.*

Whatever the cause, the "alate" spicule (fig. 9), such as is typical of *Leucosolenia*, and very frequent in more complex sponges, is stated by Sollas (9) and Ebner to have its optic axis at an acute angle with the morphological axis of the unpaired ray, in a plane

* [The acicular spicules which clothe *S. raphanus* appear completely referable to law. In the body of the radial tubes the optic axis of the triradiates is nearly parallel with the axis of the tube; this we may ascribe to compression, radial to the sponge, due to tension of the cylindrical outer surface. On the free conical ends of the tubes the optic axis is nearly perpendicular to the surface; the large acicular spicules leave the surface tangentially, and can frequently be seen to be the extremely prolonged unpaired rays of triradiates (*cf.* Ebner and others); corresponding with these relations the optic axis makes an angle of nearly or exactly 90° with the morphological axis. The fine acicular spicules leave the surface nearly vertically, and the optic axis is correspondingly coincident with their length; like the gastral rays of quadriradiates in *Ascaltis* they are to be regarded crystallographically as extremely acute rhombohedra. This is the acicular form which we should expect to find in free crystallisation of calcite, and I have shown elsewhere (19)—though suggesting a teleological explanation—that the number and size of these fine spicules vary greatly with the condition of the water.

In *L. Lieberkühnii* the optic axis is often exactly at right angles to the acicular spicules, in other cases it makes with the long axis an angle of from 70° upwards. The figures of Schulze (*S. raphanus*) and Minchin (*L. variabilis*) suggest that the larval spicules are tangential when first formed, and that some are afterwards rotated outwards. It does not seem at present possible to account for the varying angles made by the optic axis to the morphological axis of acicular spicules, without reference to cellular directive power. The giant oxeotes of the young *Leucandra aspera* especially suggest that there has been cellular determination to form a divergent brush of protective spicules. To examine this question there is necessary greater knowledge as to the mode of ontogenetic development of the canal-system, as to the mechanical strains resulting in the various stages of such development, and as to the laws governing free crystallisation of calcite in acicular forms of which the greatest length is not parallel to the optic axis.

In the club-spicules of *S. compressum* the optic axis lies in the plane of the spicule, normal to the morphological axis at its point of greatest curvature. It must be noticed that, according to the observation recorded above, the acicular spicules of the adult sponge are in this species not unicellular in formation.—*July* 23, 1898.]

perpendicular to the plane of the spicule (fig. 10, *o.a.*). The curvature of rays has been adduced by Schulze (11) as an argument against

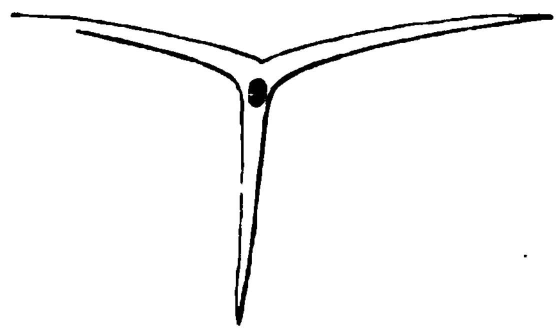


FIG. 9.—Alate spicule of *L. Lieberkühnii* (× 150).

FIG. 10.—Diagram of profile view of the spicule shown in fig. 9. The curvature is from measurement; the angle made by *oa*, the optic axis, is copied from Ebner's figure of a similar spicule, in *S. elegans*.

crystalline structure; but the form shown in fig. 9 is the trace on a cylindrical surface of three planes, which meet at equal angles in a line forming an acute angle with the axis of the cylinder, to which one plane is radial, two of them, therefore, cut the cylinder in ellipses, and one in a straight line. Investigation shows that the angle agrees with that given by Sollas and Ebner for the optic axis of a spicule of similar form.* *The alate triradiate of* Leucosolenia, *with curved rays, is therefore as accurately regular a skeleton-crystal as the equiangular triradiate of* Clathrina. Both follow the same law, that the calcareous secretion is limited by the form of the sponge to the surface of a cylinder, and crystallisation of the secretion takes place in the radial planes of a rhombohedron which are perpendicular to its three faces, the difference being only that in *Clathrina* the optic axis of the rhombohedron is radial to the cylinder, and in *Leucosolenia* it lies in a radial plane of the cylinder at an acute angle with the axis.†

* To fellow-naturalists my own method of investigation may prove helpful. Draw the spicule (fig. 9) two or three inches long on a sheet of paper; look at the diagram with the eye where the line *oa* (fig. 10) would come, and curve the paper so that it forms part of a cylinder the axis of which is parallel with the long ray. The diagram will then appear to be that of an equiangular spicule with straight rays.

† The fourth ray in *Leucosolenia* and allied sponges grows (perhaps most freely at right angles to the optic axis?) to an edge of the rhombohedron. It is often curved, a character possibly resulting from the suspension of the formative cell between it and the gastral surface. In *L. Lieberkühnii*, as observed by Minchin for *L. complicata*, its origin is on the unpaired ray. Unlike the apical ray

The young spicule of *L. Lieberkühnii* is not a skeleton, but a complete isosceles triangle (fig. 6), and the same is recorded by Breitfuss (22) for the adult spicules of his new genus *Sphenophorina*.

That natural selection has played a most important part in the arrangement of skeleton is shown clearly by the schemes in the Heterocœla, analysed by Poléjaeff (7) and Dendy (17). And if crystallisation of calcite had not been advantageous to the sponge, it would have assumed no more importance than crystallisation of urates among higher animals. But the strangely beautiful and elaborate patterns of calcareous spicules are truly crystalline, and their curves and angles, as they vary from one individual to another,* are not necessarily of appreciable advantage or disadvantage to the species in which they are found. And, in spite of the colloid nature of silica, Schulze's beautiful figures of Hexactinellid spicules prove to me that this substance has some property akin to crystallisation on the cubic system, to which the triaxon forms of siliceous spicules, and probably the tetraxon also, are to be ascribed. It remains to be shown whether the complex detail of a peacock's tail may not also be referable to a mathematical equation, rather than to the æsthetic nicety of ancestral hens. *De minimis non curat lex vitæ.*

[I find that in attempting condensation of statement I have not succeeded in completely explaining the position above advanced.

There is probably no group in which, more clearly than in the calcareous sponges, change phylogenetically progressive can be shown to result in progressive advantage to the organism, whether we consider the canal-system or the arrangement of the skeleton. It is not under dispute that, where the form of the spicule can appreciably affect the chances of survival of a sponge, there natural selection dictates to the necessary degree the form of the spicule. This it may do either directly, as appears probably the case in many acicular spicules, by developing a stubborn morphographic sense in the skeletogenous cells; or indirectly, by varying the chemico-physical conditions of the secreted fluid, so that the form of crystallisation changes. Such indirect action of selection in *A. cerebrum* may possibly explain the presence of thorns on the gastral ray, though irresponsible for their arrangement.

But, while granting fully that the most apparently unimportant variations may prove vital to the organism, it appears logically necessary that there must be variations which are unimportant, or unimportant compared with solid advantages with which they are necessarily concomitant. In the case of calcareous triradiates the solid advantage is that a mechanically useful skeletal element is

of *A. cerebrum*, its crystalline position tends to give it a laminate rather than an acicular form.

* In *Sycon raphanus* this is most markedly the case.

attained by the passive submission of a solution of carbonate of lime to forces of crystallisation; thus, with economy to the organism, very simple secreting cells produce a result similar in effect to that which would be attained by morphographic skeletogenous tissues of far more complex heredity. It would appear from comparison of the spicules of *Sycon* and *Leucascus*, *Anamixilla* and *Heteropegma* (*vide infra*), that in calcareous sponges this advantage completely outweighs any possible profit or loss resulting from slight change in angle or substitution of curved for straight rays.

It is difficult to believe that the equiangular spicule with which *Heteropegma* and *Leucandra australiensis* support their tubes, alike has induced their survival, and would cause death to the *Sycon* that should imitate them. Nor, on the other hand, can the spicule forms of *S. raphanus* be ascribed to a rigid atavistic heredity; for between individuals self-sown in the same tank, the striking differences of homologous spicules proclaim their form to be most variable. The explanation of the exact geometrical figure observed in any one case is to be sought in crystallography, and not in physiology; in the laws which we recognise as governing form in dead matter, that is, those that are apparently independent of any forces in the dimension of memory.

So far as can be judged, it seems that the change in form, which, through influence on the direction of the optic axis, results from change of stress, renders the spicule less well adapted to bear that stress; a predominant longitudinal tension placing the paired rays nearly transverse to its own direction. *A priori*, where one character in an organism is physically a function of another character in the organism, the variation of one only can be exactly correlated to the needs of the organism. If both are important, the adaptation of neither can be perfect; according as either is predominant, the other exhibits phenomena which do not conduce to survival of the variety in which they are noted.

The calcite crystal may be compared to a symbiotic organism; its characters within certain limits of saliency are subordinated to the needs of the organism within whose tissues it finds a welcome. But the number of mesenteries in *Adamsia* is dictated by its own history, and not by the mode of life of the crab which carries it; so the angles of a triradiate calcareous spicule are dictated by the properties of calcite, and, within a considerable range, would appear neither to influence nor be influenced by selective mortality in the species among which it occurs. To hold that this view is unjustified it seems necessary to suppose that individuals of *A. cerebrum*, in whom gastral thorns were not parallel to the facial rays of the spicule which bears them, have consequently perished and been unable to *procreate their race.—July* 23, 1898.]

II. *Classification.*

Minchin, in 1896 (20) and 1897 (21), recorded that the larva of *L. variabilis* is an amphiblastula, as shown by Metschnikoff for *L. Lieberkühnii*; and that that of the species *coriacea*, *cerebrum*, *reticulum*, and *contorta* (*sensu* Bwk.) is a parenchymula, as shown by Miklucho-Maclay (1) for *blanca* (confirmed by Minchin), and by Schmidt (2) and Metschnikoff (6) for *primordialis* and *clathrus*. He pointed out that the first spicules to appear in the amphiblastula larva are acerates, in the parenchymula larva triradiates. He found that in *L. botryoides*, *variabilis*, *complicata*, and *Lieberkühnii* the nucleus of the collar-cell is distal, as in *Sycon*; in the species with parenchymula larvæ it is basal.*

On these important observations he divides the Homocœla into two families.

(1) *Clathrinidæ* (*Clathrina clathrus*, *coriacea*, *cerebrum*, *reticulum*, *contorta*, and *Ascandra falcata*) with reticulate external form, equiangular triradiate systems, collar-cells with basal nuclei, parenchymula larva, and triradiates the first spicules to appear.

(2) *Leucosoleniidæ* (*Leucosolenia botryoides*, *complicata*, *Lieberkühnii*, *variabilis*, and *Ascyssa* (?)) with erect or arborescent form, sagittal triradiate systems, collar-cells with terminal nuclei, amphiblastula larva, and monaxon spicules the first to appear. Such Sycons as *S. raphanus* he derives from the Leucosoleniidæ, but leaves it an open question whether some Heterocœla may not be derived from the Clathrinidæ.

I propose to emphasise very considerably the lines he has indicated, and particularly the suggestion (originally made by Keller (5)) that *Leucosolenia* is closely allied to the Sycons. The classification of sponges according as they are homocœl or heterocœl, is a physiological classification by the most essential and active organ of the sponge; I have always regarded it as no more satisfactory than classifying higher animals according to whether they walk, swim, or fly. Dendy has already pointed out the close resemblance of his Leucascidæ with the reticulate Ascons ((17) pp. 166, 190, 249, &c.); I find† that *Heteropegma nodus Gordii* of Poléjaeff has basal nuclei to the collar-cells and that the optic axis is perpendicular to the plane of the triradiates, while *Anamixilla* is opposed to it in both these characteristics. [I have also confirmed on *L. Lieberkühnii*, Minchin's observation as to the distal nuclei in *Leucosolenia*.] There is perhaps

* It may be remembered that I stated to the Society, in 1892, that "the nucleus of Homocœla is generally basal, whereas in the Heterocœla, contrary to current statement, it is almost always distal" (16), p. 479; *cf.* also (19), p. 21.

† *I owe to the kindness of* Dr. Vosmaer and Dr. Poléjaeff the opportunity of *investigating type-slides of the* "Challenger" sponges.

little reason for supposing *Calcarea* to be a natural class; but retaining it provisionally I propose to divide as follows :—

Class.—CALCAREA.

Sub-Class I.—*Calcaronea*, nov.

The nucleus of the collar-cells and of the flagellate cells of the larva is distal, and the flagellum arises from it directly. The larva is an amphiblastula. The first spicules to appear are oxea, generally (always ?) lance-headed, the triradiates are typically alate* and the optic axis is rarely perpendicular to the plane of the spicule. The pylocyte is annular† and generally lies at the bottom of a funnel-shaped depression or afferent canal. Branching of the sponge takes place typically nearly at right angles to a growing axis, giving rise to stolonate and arborescent forms. The gastral fourth ray of a quadriradiate spicule rarely rises perpendicularly from the meeting point of three rays. Lance-headed oxeotes are frequently present in the adult. The sponges never show a coral-red or sulphur-yellow colour.

Order 1. *Asconida*, H. (s.m.). The central cavity is in the adult lined with collar-cells and communicates with the exterior directly by pylocytes in its walls.

Fam. *Leucosolenidæ*, Minchin. Genus 1. *Leucosolenia*, Bwk. *emend.* Minchin. Genus 2. *Ascyssa*, H.

Order 2. *Sycettida*, nov. The central cavity is in the adult not lined with collar-cells and does not communicate with the exterior directly by pylocytes in its walls.

Fam. 1. *Sycettidæ*, Dendy.
„ 2. *Grantidæ*, Dendy.
„ 3. *Heteropidæ*, Dendy.
„ 4. *Amphoriscidæ*, Dendy (s.s.).

Sub-Class II.—*Calcinea*, nov.

The nucleus of the collar-cells and (? ?) of the flagellate cells of the larva is basal, and the flagellum does not arise from it directly. The larva is a parenchymula. The first spicules to appear are triradiates, the triradiates are typically equiangular and the optic

* *I.e.*, with paired angles, from the resemblance of the oral rays of such spicules to the two wings of a flying bird. [I propose the corresponding term "caudate" for equiangular sagittal spicules.]

† "Pylocyte" = the cell surrounding a prosopyle, leaving "porocyte" = the cell surrounding a pore. I have observed this in *Leucosolenia Lieberkühnii*, *Sycon raphanus*, *Sycon compressum*, and *Leucandra aspera*. *Cf.* Dendy on *Leucosolenia stolonifera* ((14), p. 25), and *Granlessa intusarticulata* ((17), fig. 30), and Poléjaeff on *Grantia tuberosa* ((17), pl. 3, fig. 7).

axis is generally perpendicular to the plane of the spicule.* The pylocyte has as yet only been investigated in homocœl species, it is there amœboid, and perforates the entire sponge-wall without any afferent funnel-shaped depression lined by other ectocytes. Branching of the sponge is typically dichotomous or umbellate, with frequent anastomoses, giving rise to reticulate growths supported on solid stalks often of obvious length in the adult. The fourth ray of a quadriradiate spicule generally (or always) rises perpendicularly from the meeting point of three rays. Lance-headed oxeotes are rarely (or never) present, either in larva or adult. Most species show varieties which are coral-red and sulphur-yellow.

Order. 1. *Ascettida*, nov. No quadriradiate spicules are present.

Fam. 1. *Clathrinidæ*, Minchin (s.s.). There is no distinct pore-bearing dermal membrane.

Genus 1. *Clathrina*, Gray. With knobbed ends to the spicules. Sp.: *C. clathrus.*

Genus 2. *Guancha*, M.M. With pointed ends to the spicules. Sp.: *G. blanca, coriacea.*

Fam. 2. *Leucascidæ*, Dendy. A distinct pore-bearing dermal membrane is present.

Genus 1. *Leucascus*, Dendy.

Order 2. *Ascaltida*, nov. Quadriradiate spicules are present.

Fam. 1. *Reticulatæ*, Dendy (s.s.). The radial arrangement of the flagellate tubes is only pronounced near the cloaca.

Genus 1. *Ascaltis*, H. (s.m.) The flagellate epithelium is not pouched deeply into the wall of the sponge. Sp.: *A. cerebrum*, *A. reticulum*, *A. primigenia* (*Leucetta primigenia*, H.=*Leucosolenia ventricosa*, Dendy ?).

Genus 2. *Ascandra*, H. (*sensu* Minchin). The flagellate epithelium is pouched deeply into the wall of the sponge. Sp.: *A. falcata.*

Fam. 3. *Heteropegmidæ*, nov. The flagellate tubes are completely radial in arrangement.

Genus 1. *Dendya*, nov. The ends of the branches, even when united, are distinguishable as separate prominences on the external surface, and there is no true dermal membrane or cortex. Sp.: *Dendya tripodifera*, (= *Leucosolenia tripodifera* (Carter) Dendy (14)).

* [In a specimen of *G. blanca* I find the optic axis at the base of the cup 9° below the external perpendicular on the spicule-plane, while on the solid stalk it rises 33° above the perpendicular. This would seem to be consonant with the probable direction in these parts of the line of maximum thrust, and to account for the abnormal "horn" spicules described by Metschnikoff (5) and Poléjaeff (7)]

Genus 2. *Heteropegma*, Pol. The external surface is a cortex with dermal skeleton, and the ends of the branches are indistinguishable as separate prominences. Sp.: *Heteropegma nodus gordii.*

(From Dendy's illustration it would seem that his *Leucandra australiensis* ((17) Fig. 17) should form the type of a third genus in this family, with a dermal membrane but no cortex.)

It is impossible to estimate the degree of relationship between Calcaronea and Calcinea until the histology of siliceous sponges is better known. The fact that the skeleton is in both groups composed of calcite seems little evidence of common origin; since the researches of Ebner and Minchin suggest that they separated before the calcite took the form of spicules. The mode of attachment of the flagellum would seem necessarily perfected so soon as the mode of nourishment by aquiferous canals was established for the race; on this reasoning it would seem not improbable that the phylum Sponges may be composed of two classes, the Basinucleata and the Apicinucleata; or the Hexactinellida may be a third class of equal value. The Spongillidæ and Spongida are the two groups among Demospongiæ which would appear to offer themselves for union with the Calcinea, as having basal nuclei to their collar-cells and larvæ completely flagellate; for the Apicinucleata we know of distal nuclei in the Calcaronea, *Halichondria* (18) and probably (reasoning from Schulze's figures) *Chondrosia* (3), *Corticium* (6), *Halisarca* (3).

The Spongillidæ and Spongida are both anomalous groups, and the Demospongiæ would be rather more homogeneous if they were removed, and scarcely more heterogeneous by the admission of the Calcinea. But our knowledge is as yet too imperfect to propose such a classification.

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"The Influence of Removal of the Large Intestine and increasing Quantities of Fat in the Diet on general Metabolism in Dogs."

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(Abstract, published during the Vacation.)

In this research it was intended, by comparing the results obtained in dogs on a given diet with the same animals after the removal of the large intestine, to study more carefully the functions and any influence the absence of the large intestine might have on general metabolism.

In one dog a little more than the middle third of the large intestine was removed, while in the other two dogs the total length of the large intestine, together with the cæcum, was extirpated. The dogs were fed after recovering from the operation on meat and biscuit, to which varying quantities of fat were added. The meat employed was preserved by sterilising minced meat in separate weighed out portions sufficient for each day.

In all the experiments the nitrogen and fat in the diet were analysed, and in two dogs quantitative analyses were made of the carbohydrates in the diet and fæces. It was found on the above diet that no carbohydrates were obtainable in the fæces either in normal dogs or in those in which the large intestine had been removed, so that it can be concluded that the large intestine has no action on the carbohydrate absorption, and in subsequent experiments it was not investigated.

The first step was to investigate the effect of an increasing quantity of fat on a staple diet in normal animals, so as to compare that with the results obtained after the removal of the large intestine.

In dog 1 (Table III) it is seen on the same carbohydrate and proteid diet when the quantity of fat is increased from 12·04 grams to 32·04 grams, the average quantity of urine fell from 118 c.c. to 89 c.c., and this decrease in quantity was accompanied by a slight increase in the specific gravity of 1058 to 1060. In consequence of the proteid sparing action of the fat the quantity of nitrogen eliminated fell from 4·457 grams to 3·575 grams. On increasing the diet still further to 62·04 grams, the quantity of urine was only 70 c.c., the specific gravity remaining 1060, the nitrogen being slightly decreased in quantity to 3·362 grams.

As far as the fæces are concerned, their daily quantity increased from 18·61 grams to 20·42 grams and 22·70 grams. Together with the increased quantity of the fæces the nitrogen daily eliminated also increased in quantity from 0·351 gram to 0·412 gram and 0·469 gram. As one would naturally expect, with the increased quantity of fat in the diet, the fat in the fæces increased from 0·733 gram to 0·971 gram

Table III.—The Effect of an Increasing Quantity of Fat in the Diet on Normal Metabolism, giving the Averages of Periods of Four Days Duration.

No.	Weight.	Duration of observation.	Diet.		Urine.			Fæces.			Absorbed.	
			N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	days.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
1*a*	4·59	4	4·82	12·04	118	1058	4·457	18·61	0·351	0·733	92·71	93·91
b	4·59	4	4·82	32·04	89	1060	3·575	20·42	0·412	0·971	91·45	96·97
c	4·63	4	4·82	62·04	70	1060	3·362	22·70	0·469	1·264	90·26	97·93
2*a*	6·41	4	8·00	15·20	119	1054	6·127	31·67	0·696	0·776	91·29	94·68
b	6·41	4	8·00	15·20	108	1059	5·815	33·62	0·799	0·898	90·01	94·09
c	6·59	4	8·00	65·19	74	1061	3·858	36·26	0·901	2·249	88·73	96·55

and 1·264 gram. In consequence of the increase of the nitrogen in the fæces, the apparent absorption of nitrogen per diem fell from 92·71 per cent. to 91·45 per cent. and 90·26 per cent., while, on the other hand, in spite of the increased quantity of fat in the fæces, the absorption of fat rose from 93·91 per cent. to 96·97 per cent. and 97·96 per cent. by increasing the fat in the diet.

In dog 2 during the three periods examined, the first two were on the same quantity of fat, and the results found correspond with those found in dog 1.

The next observation (Table V) was made on a dog in which a little more than one-half of the large intestine was removed. It is seen that on increasing the fat from 11·73 to 36·73 grams the quantity of urine fell from 172 c.c. to 169 c.c., the specific gravity falling from 1035 to 1031. The nitrogen also fell from 5·596 grams to 4·991 grams. On still further increasing the fat to 51·73 grams, the average quantity of urine fell to 112 c.c. with a specific gravity of 1048, the quantity of nitrogen being 4·680 grams when four days were analysed. As far as the quantity of fæces is concerned, during two of the periods the dog did not pass its fæces daily, so that the average can only be made approximately.

The increase in the quantity of fat is seen in this case to cause no increase in the quantity of fæces, although the total quantity of fæces are more than the previous normal dogs would lead one to suppose ought to occur on that diet. The total nitrogen in the fæces also is increased in quantity, although no increase occurs with the addition of the fat. The fat analysis in the three periods remained practically the same; in consequence of this, the absorption is not altered by increasing the fat. The total absorption of proteid varies from 86·91 per cent. to 89·85 per cent., that is to say less than the normal dogs, while the fat absorption rose from 86 per cent. to 97 per cent., practically the same as is found when the large intestine is intact.

In the next two dogs the entire large intestine was removed, and the result obtained is seen in Table VIII.

In dog 4, four periods were analysed, each having four days duration. During periods *a* and *b* the quantity of fat was 9·71 grams, while in periods *c* and *d* the quantity was 29·71 grams.

It is seen that the quantity of urine during the periods *a* and *b* varied very considerably, and the same occurred with the increased quantity of fat. The cause of this variance it would be impossible to explain; the tendency, however, is for the quantity to slightly decrease with the increased quantity of fat, and specific gravity to do the same.

At the same time the decrease in quantity on increasing the fat is not nearly so marked as in the normal dogs. The increase of fat in the diet, however, brings out the same proteid sparing action as the

Table V.—The effect on increasing the quantity of Fat in the Diet on General Metabolism is seen after partial removal of Large Intestine. Averages of Three Periods of Four to Five Days Duration. Figures in ( ) not complete averages owing to an analysis in each case being lost.

No.	Weight.	Duration of observation.	Diet.		Urine.			Fæces.			Absorbed.	
			N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	days.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	p. c.	p. c.
3*a*	6·18	5	6·05	11·73	172	1036	5·596	39·95	0·792	(1·217)	86·91	(86)
b	6·23	4	6·05	33·73	169	1031	4·991	36·39	0·614	1·437	89·85	96·09
c	6·75	5	6·05	51·73	112	1048	(4·680)	38·31	0·624	(1·492)	89·69	(97)

Table VIII.—The effect of an increasing quantity of Fat in the Diet on General Metabolism in two Dogs in which the entire large intestine had been previously removed. Averages of Four Periods of Four Days, and One of Five and One of Two Days duration. Figures in ( ) not complete averages.

No.	Weight.	Duration of observation.	Diet.		Urine.			Fæces.			Absorbed.	
			N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	days.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	p. c.	p. c.
4*a*	4·05	4	6·80	9·71	191	1025	4·445	75·55	1·064	0·777	84·36	92·00
b	4·08	4	6·80	9·71	341	1014	4·374	74·71	1·081	0·605	84·11	93·77
c	4·17	4	6·80	29 71	207	1019	3·243	83·78	1·088	0·769	84·01	97·41
d	4·29	4	6·80	29·71	198	1023	2·965	93·49	1·095	0·873	84·14	97·06
5*a*	6·30	5	6·26	11·55	93	1044	4·376	74·16	1·031	(0·553)	83·54	(95)
b	6·58	2	6·26	41·55	91	1028	2·415	59·38	0·606	0·819	90·33	98·03

quantity of nitrogen in the urine falls from 4·445 grams and 4·374 grams to 3·243 grams and 2·965 grams.

The quantity of fæces during the different periods compared very much better than the quantity of urine, and on a small fat diet the quantity was 75·55 grams and 74·71 grams, that is to say, more than double the quantity on the same diet in normal dogs. On increasing the fat in the diet the quantity rose to 83·78 grams and 83·49 grams, so that we have here the same as in the normal dogs an increase in the quantity of fæces caused by increasing the quantity of fat in the diet. At the same time the total quantity of fæces is very much in excess of that on the same diet in normal dogs.

. The nitrogen in the fæces during the periods *a* and *b* was 1·064 grams and 1·081 grams, nearly three times the amount obtained in normal dogs, while on increasing the fat it only increased slightly to 1·088 grams and 1·095 grams. The fat in the fæces during periods *a* and *b* was 0·777 gram and 0·605 gram, and on increasing the fat in the diet it rose to 0·769 gram and 0·873 gram, so that the total quantity of fat obtained in the fæces in dogs in which the large intestine had been removed was almost the same as in the normal dog, and the increase of fat in the diet caused an increase in the quantity of fæces.

Now, turning to the absorption as indicated by the quantity found in the fæces, we see while the normal dogs absorbed over 90 per cent. of the nitrogen given, in the absence of the large intestine only 84 per cent. was absorbed, and that the increase in the quantity of fat given caused very little decrease in the absorption of nitrogen per cent. The absorption of fat in the normal dogs varied from 93 per cent. to 97 per cent., while in these dogs we see that it varies from 92 per cent. to 97 per cent., so that as far as the absorption of fat is concerned the large intestine plays no part.

In dog 5, in period *b*, when 41·55 grams of fat were given, the animal refused to take its food, so that only two days' analysis were able to be given; but the results obtained in dog 5 correspond with those in dog 4, and it was found in other dogs that it was impossible to increase the quantity of fat in the diet in the absence of the large intestine, as the animals invariably went off their feed.

The above results showed that the removal of the large intestine has a great influence on the quantity of fæces, it will be as well now to discuss the change in the quantity of water eliminated in the fæces and its percentage composition.

On comparing the averages of the above dogs we see (Table XIV) that in the normal dog the quantity of fæces varies from 18·61 grams to 36·26 grams, while when the large intestine is partially removed the quantity is slightly increased, although the small quantity found may *be partly* explained by constipation. On removal of the whole of the

Table XIV.—The Influence of an increasing Quantity of Fat in the Diet on the Quantity of Water in the Fæces.

No.	Duration of observation.	Diet.		Fæces.		
		N.	Fat.	Quantity.	Water.	
					Total grams.	per cent.
	days.	grams.	grams.	grams.		
		Average of Two normal Dogs.				
1*a*	4	4·82	12·04	18·61	12·79	70·78
b	4	4·82	32·04	20·42	13·79	67·95
c	4	4·82	62·04	22·70	14·32	67·87
2*a*	4	8·00	15·20	31·67	19·90	63·26
b	4	8·00	15·20	33·62	22·03	64·60
c	4	8·00	65·19	36·26	23·67	60·52
		Average of partial Removal of Large Intestine.				
3*a*	5	6·05	11·73	39·95	25·44	50·96*
b	4	6·05	36·73	36·39	26·44	54·59*
c	5	6·05	51·73	58·31	28·30	72·46
		Average of total Removal of Large Intestine.				
4*a*	4	6·80	9·71	75·55	58·11	76·59
b	4	6·80	9·71	74·71	56·87	76·26
c	4	6·80	29·71	83·78	66·56	79·45
d	4	6·80	29·71	83·49	66·09	79·13
5*a*	5	6·26	11·55	74·16	58·57	79·09
b	2	6·26	41·55	59·38	49·49	83·62

* One day passed no fæces.

large intestine, on the other hand, the quantity of fæces varies from 75 grams to 83 grams; that is to say, was very markedly increased.

The quantity of water in the normal fæces per diem is increased on increasing the quantity of fat in the food.

In dog 1 it rose from 12·79 grams to 14·32 grams. After removing the large intestine there is also an increase in the quantity of water on increasing the quantity of fat in the diet, for it rose from 58·11 grams and 56·87 grams to 66·56 grams and 66·09 grams by increasing the fat in the diet. Also the percentage quantity eliminated with the fæces is enormously increased by removal of the large intestine. Even the partial removal of the large intestine in dog 3 shows a very marked increase in the quantity of water per diem. The percentage of water in the fæces in the normal dogs slightly falls with the increased quantity of fat in the diet, whilst instead of falling the percentage increases after the removal of the large intestine. In the normal dog 70·78 per

Table XX.—The Effect of the Removal of the Large Intestine on the breaking up of Fat in the Alimentary Canal. Composition of Fat of Fæces.

No.	Duration of observation.	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.		Cholesterin.	
		Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
	days.										
Average of Two normal Dogs.											
1	4	1·264	100	0·139	10·03	0·902	65·47	0·233	14·22	0·154	11·80
2	3	2·325	100	0·314	13·63	1·359	59·45	0·591	24·12	0·061	2·80
Average of partial Removal of Large Intestine.											
3	3	0·972	100	0·145	13·16	0·514	54·74	0·178	15·91	0·145	17·96
Average of Two Dogs after complete Removal of the Large Intestine.											
4	4	0·801	100	0·124	15·69	0·479	62·11	0·180	19·31	0·025	3·84
5	3	0·715	100	0·116	16·20	0·413	59·73	0·116	13·91	0·069	10·11

cent. of water was eliminated with 12 grams of fat, on increasing the fat to 62 grams only 67·87 per cent. of water is eliminated.

In the dog in which the large intestine was entirely removed, dog 4 with 9·71 grams of fat, the fæces contained 76·59, but on increasing the fat to 29·71 grams the percentage of water increased to 79 grams instead of falling as in normal dogs.

The effect of removal of the large intestine on the breaking up of fat in the alimentary canal was next investigated (Table XX).

In the above table the quantities of neutral fat, free fat acids, fat acids as soaps and cholesterin are given, and their percentage composition taking the total ether extract as 100. It is seen in all the dogs that the quantity of free fat acids is very much greater than that of the neutral fat; the quantity of fat acids as soaps and neutral fat correspond very much in percentage composition.

As far as the total quantity is concerned, that varies with the diet, but on the whole is comparable, so that one can conclude that the removal of the large intestine has no action in stopping either the breaking up of fat or the formation of soaps in the alimentary canal.

When we turn to the cholesterin, however, we find that there is a difference. The normal dog 1 excreted 0·154 gram and 2 only 0·061 gram of cholesterin. After partial removal of the large intestine the quantity of cholesterin corresponded very much with the normal dog 1, being 0·145 gram, while in the case of both dogs, when the large intestine was entirely removed, the quantity of cholesterin was very much less (in dog 4 only 0·025 gram, and in dog 5 0·069 gram); so that one may consider that the removal of the large intestine causes a decrease in the quantity of cholesterin, and this is probably explained by the loss of so much secreting surface in the intestine as would occur when the large intestine is removed.

The colouring matter in the fæces was also investigated in the normal dogs, and found in all cases to contain no bile pigment but marked quantities of urobilin, while in the case of the dogs in which the large intestine had been removed this was not always the case.

As far as the contents of the intestine or its walls are concerned the animals were killed with chloroform, and the presence of urobilin looked for throughout the intestine. In the normal dogs in the great majority of cases no trace of urobilin could be detected above the ileocæcal valve. The contents of the small intestine only gave the bile reaction.

In dog 5 the small intestine having been joined 6 cm. from the anus, a slight urobilin reaction was obtained as high as 35 cm. from the anus, the bile reaction as far as 78 cm. In this case evidently urobilin formation was taking place in the small intestine comparatively speaking high up.

In dog 4 only 4 cm. of rectum was left, and only in this part was

any urobilin reaction obtained, so that in this case as in normal dogs only the large intestine formed urobilin.

The next step in the investigation was to see the influence of diet and removal of the large intestine on the sulphates in the urine.

In Table XXVI it is seen that on increasing the fat in the diet, as the quantity of nitrogen decreases in the urine the quantity of total sulphates do the same; with 12 grams of fat a dog eliminating 0·637 gram of sulphates, with 32 grams of fat 0·544 gram, and with 62 grams only 0·521 gram. On the other hand it is seen that this steady increase in the quantity of fat accompanied by the decrease in the sulphates, is not due to a diminution in the quantity of aromatic sulphates, but of the alkaline sulphates, the aromatic sulphates remaining throughout practically the same, 0·064 gram. In consequence of the decrease in the alkaline sulphates the ratio A to B is decreased, so if one only referred to the ratio one would believe that there was an increase in the intestinal putrefaction, while on the other hand in reality there is no increase; if anything a decrease, as brought out in dogs 1 and 2, where the aromatic sulphates are not increased but, if anything, decreased.

In dog 3, in which the large intestine was partially removed, the influence of fat in the diet on the sulphates is the same as in normal dogs, and the quantity of aromatic sulphates corresponds with that found in normal dogs, so that one can say that there is no increase or decrease in intestinal putrefaction caused by partial removal of the large intestine.

In dog 4 after complete removal of the large intestine it was seen the quantity of total sulphates corresponds with that found in normal dogs, only there is a marked decrease in the quantity of aromatic sulphates to half the normal. In consequence of this the ratio is very much increased, and, both by the ratio, increase, and the total decrease in aromatic sulphates, one sees clearly that intestinal putrefaction is very much diminished.

In dog 5 the same is brought out, but not to such a great extent, and, as is already seen, dog 5 was an exception as far as the urobilin was found to occur in the small intestine some way up.

In conclusion we may say that this research has led to some interesting results. That, as far as the large intestine influences the absorption of food stuffs, it has no action whatever on the carbohydrates of the diet, but its absence causes a marked decrease in the absorption of proteids from 93 per cent. to 84 per cent. The fat, on the other hand, is absorbed in practically the normal amounts, and it is found that the breaking up of fat continues the same when the large intestine is absent. The water of the fæces is increased in total quantity, although the percentage of water increases with an increased fat diet, instead of *decreasing* as in normal dogs. The total quantity of fæces is also

Table XXVI.—The Alkaline and Aromatic Sulphates. The Average Results of Normal Dogs as compared with Partial and Complete Removal of the Large Intestine, showing the Influence of an Increased Quantity of Fat in a fixed Proteid and Carbohydrate Diet. No. 1 and No. 2, Normal Dogs; No. 3, Partial Removal of Large Intestine; and Nos. 4 and 5, Complete Removal of the Large Intestine.

No.	Duration of observation.	Diet.		Quantity.	N.	Total.	Sulphates.		A : B.
		N.	Fat.				Alkaline A.	Aromatic B.	
	days.	grams.	grams.	c.c.	grams.	gram.	gram.	gram.	Ratio.
1*a*	4	4·82	12·04	118	4·457	0·637	0·573	0·064	9 : 1
b	4	4·82	32·04	89	3·573	0·544	0·479	0·065	8 : 1
c	4	4·82	62·04	70	3·362	0·521	0·465	0·056	6·5 : 1
2*a*	4	8·00	15·20	119	6·127	0·662	0·558	0·076	7 : 1
b	4	8·00	15·20	108	5·815	0·679	0·597	0·082	7 : 1
c	3	8·00	65·19	74	3·858	0·445	0·377	0·066	6 : 1
3*a*	5	6·05	11·73	172	5·596	0·768	0·685	0·072	9 : 1
b	4	6·05	36·73	169	4·991	0·727	0·649	0·077	8 : 1
c	4	6·05	51·73	112	4·680	0·681	0·615	0·066	9 : 1
4*a*	4	6·80	9·71	191	4·445	0·598	0·582	0·034	17 : 1
b	3	6·80	9·71	341	4·374	0·584	0·588	0·026	22 : 1
c	4	6·80	29·71	207	3·243	0·437	0·405	0·031	13 : 1
d	3	6·80	29·71	247	3·510	0·468	0·432	0·035	12 : 1
5*a*	5	6·26	11·55	93	4·376	0·444	0·382	0·052	7 : 1
b	3	6·26	41·55	88	2·526	0·232	0·201	0·031	6·6 : 1

increased on the same diet as that in the normal dogs, and the cholesterin is decreased.

That the formation of urobilin in the fæces is diminished in the absence of the large intestine; the sulphates vary the same as the normal as regards those combined with the alkalis, while those combined with the aromatic substances are markedly diminished, showing that intestinal putrefaction is decreased.

"Further Observations concerning the Relation of the Toxin and Anti-Toxin of Snake-Venom." By CHARLES J. MARTIN, M.B., D.Sc., Acting Professor of Physiology in the University of Melbourne. Communicated by W. D. HALLIBURTON, F.R.S. Received August 23, 1898, and published during the Vacation.

The discrepancy between the quantities of anti-venene required to neutralise a given dose of venom when they are (1) previously mixed outside the body, and (2) simultaneously injected under the skin in different parts of the body, has been drawn attention to by Fraser and myself. My experience coincides with Fraser's* upon this point, viz., that it requires at least 10—20 times as much anti-venene to counteract a given dose of venom when they are injected separately, but at the same time, as is necessary to effect this if they are mixed together prior to injection.

Sometimes, however, the quantity necessary by simultaneous but separate injection may be much greater; in one of Fraser's experiments 1000 times as great.† Moreover, there is no constant ratio between the amounts necessary under the two conditions, as will be seen from the experiments tabulated below (Series A). In this series, experiments 1—4, in which increasing doses of venom were employed, show that 0·5 c.c. of the particular sample of serum used was more than adequate to prevent a fatal result when previously mixed for fifteen minutes at temperature 13° C. with 0·5 c.c. of a solution containing 0·0001 gram of the venom per c.c. As 0·00003 gram per kilo was found to be the minimal fatal dose of this poison, one may be sure that under these conditions 0·5 c.c. of the serum is adequate to neutralise more than 0·00002 gram of the venom, that is, 0·00005 gram *minus* one fatal dose.

In experiments 5—12, 0·00005 gram of venom per kilo. was injected in each case, and increasing amounts of serum separately, but at the same time. Under these conditions, every quantity less than 8 c.c., that is, sixteen times as much as is fully adequate to prevent any symptoms when brought directly into contact with the poison before

* 'Nature,' April 23, 1896.

† *Loc. cit.*, p. 594.

injecting, failed to counteract the venom. Even as much as 9 c.c. and 15 c.c. was equally useless, although the animals in the experiments in which 8 c.c. and 10 c.c. were employed lived.

Series A.

	Amount of venom per kilo. of rabbit.	Amount of serum per kilo.		Result.
1	0·00005 gram	0·5 c.c.	Mixed together for 15 minutes prior to injection.	Lived.
2	0·00006 "	0·5 "		Lived.
3	0·00007 "	0·5 "		Died.
4	0·00008 "	0·5 "		Died.
5	0·00005 "	5·0 "	Injected simultaneously but separately.	Died.
6	0·00005 "	6·0 "		Died.
7	0·00005 "	7·0 "		Died.
8	0·00005 "	8·0 "		Lived; very ill for 3 days.
9	0·00005 "	9·0 "		Died.
10	0·00005 "	10·0 "		Lived.
11	0·00005 "	15·0 "		Died.
12	0·00005 "	20·0 "		Lived.

The further experiments detailed in the present paper afford a reasonable interpretation of the very different efficacy of anti-toxic serum under these two conditions. They are also an additional confirmation of the conclusions regarding the direct chemical nature of the antagonism between the toxins and anti-toxins of diphtheria and snake-poison respectively, which were drawn by Cherry and myself in a recent paper.* Moreover, some inferences which seem to me to be necessitated by the experimental results are of practical importance in the treatment of snake-poisoning, and not devoid of interest in their bearing upon the relations of toxins and anti-toxins in general.

The experiments arranged in tabular form below were made with the object of obtaining definite data concerning the proportions of anti-toxin to toxin necessary to save an animal under the following three conditions:—

* 'Roy. Soc. Proc.,' vol. 63, p. 420, 1898.

(1) Mixed together prior to injection.
(2) Injected simultaneously, the anti-toxin into a vein, and the venom subcutaneously.
(3) Injected simultaneously, but separately, under the skin.

The venom of *Hoplocephalus curtus*, the Australian tiger-snake, was used. After weighing the dried venom, it was dissolved in enough 0·9 per cent. NaCl solution for 1 c.c. to contain 0·0001 gram of the venom. This solution was heated momentarily to 90° C. in order to destroy one of the poisonous constituents which coagulates at 85° C.* The poisonous proteose remaining produces the same symptoms as cobra poison, and is very probably identical with the principal poisonous constituent in that venom.

The anti-venene was prepared by Dr. Calmette. Two quite different serums were used. For the experiments in Series A above, samples dated November, 1896, were employed, and for the experiments in Series B samples bearing date December, 1897. The anti-toxic value of the former, according to Behring's method of notation, I found to be 1/50th of a normal unit per c.c.; of the latter 1/200th of a normal unit per c.c.†

The control experiments are in Table I. Here the same proportions of venom to body weight, as employed in the experiments in Tables II, III, and IV, were injected, but no serum given. The effect of these doses of venom upon the rectal temperature and the time they took to kill is shown for comparison. From these experiments it is seen that 0·00003 gram venom per kilo. is just on the margin of fatality. From other experience I have found that this quantity generally kills.

II and III show parallel series of experiments. The amount of anti-venene per kilo. remains constant, but the quantities of venom in each case increase from 0·00003 to 0·00008 gram per kilo. In II the venom and anti-venomous serum were mixed together in a glass and allowed to remain in contact for fifteen minutes at temperature 13° C. prior to injection. In III the anti-venene was injected into the jugular vein at the same time that the venom solution was placed under the skin. II and III are similar in every other respect. The solution of venom and the serum used were the same, and the experiments were made at the same time.

The two experiments in IV, made with the same venom solution and serum, show for comparison the result of injecting the venom and anti-

* C. J. M., 'Roy. Soc. N.S.W. Proc.,' August, 1896.

† For the latter sample I am indebted to the kindness of Dr. Calmette. The anti-toxic value mentioned above refers to the serum as it arrives in Australia, and titrated against the venom of *Hoplocephalus curtus*, heated to 90° C. I believe, from Dr. Calmette's statement, that the serum must be much more effective against cobra poison, or else that it deteriorates before reaching me.

Series B. Table I.—Control Experiments with Venom Solution only.

	Weight of rabbit in grams.	Amount of venom per kilo.	Amount of serum per kilo.	Temp. at time of injection.	Result.						
					Temp. 12 hours after.	Temp. 24 hours after.	Temp. 36 hours after.	Temp. 48 hours after.	Temp. 60 hours after.	Temp. 72 hours after.	
		gram.		°	°	°	°	°	°	°	
1	1300	0·00003	..	39·5	39·2	38·3	38·3	36·5	..	..	Died in 50—54 hours.
2	1230	0·00003	..	39·7	39·7	38·7	38·7	38·8	39	39·7	Lived.
3	1120	0·00005	..	39·8	38	..	..	..	..	..	Died 26 hours.
4	1190	0·00006	..	39·2	..	..	..	..	..	..	Died 13 hours.
5	1150	0·00008	..	40·1	..	..	..	..	..	..	Died 11 hours.

Table II.—In which the Venom Solution and Anti-venene were mixed together for 15 Minutes prior to Injection under the Skin.

		gram.	c.c.	°	°	°	°	°	°	°	
6	1230	0·00003	2	39·8	39·6	39	39·8	39·8	..	..	Lived.
7	1300	0·00004	2	39·8	39·7	39·6	39·8	39·9	39·8	..	Lived.
8	1330	0·00005	2	40	39·8	39·6	38·2	35	..	..	Died 53 hours.
9	1108	0·00006	2	40·2	39·4	33·4	39·8	39·4	38·5	34	Died 75 hours.
10	1112	0·00007	2	39·5	37·5	36·0	..	..	..	..	Died 27 hours.
11	1190	0·00008	2	39·25	37·6	36·4	..	..	..	..	Died 27½ hours.

Table III.—In which the Anti-venene was injected into the Jugular Vein at the same time that the Venom Solution was introduced under the Skin.

	Weight of rabbit in grams.	Amount of venom per kilo.	Amount of serum per kilo.	Temp. at time of injection.	Result.						
					Temp. 12 hours after.	Temp. 24 hours after.	Temp. 36 hours after.	Temp. 48 hours after.	Temp. 60 hours after.	Temp. 72 hours after.	
		gram.	c.c.	°	°	°	°	°	°	°	
12	1230	0·00003	2	39·5	39·5	39·6	39·6	39·7	..	39·6	Lived.
13	1230	0·00004	2	39·8	39·6	39·4	39·9	40	..	39·9	Lived.
14	1260	0·00005	2	39·5	39·5	38·5	39·5	39·5	..	39·6	Lived.
15	1240	0·00006	2	39·8	37·2	35·0	..	..	..	..	Died 26 hours.
16	1300	0·00007	2	39	36·8	30	..	..	..	..	Died 24½ hours.
17	1310	0·00008	2	40	35	..	..	..	..	..	Died 20 hours.

Table IV.—In which the Venom and Anti-venene were injected separately but simultaneously under the Skin.

		gram.	c.c.	°	°	°	°	°	°	°	
18	1100	0·00005	10	40	36·2	..	..	..	..	..	Died 15 hours.
19	1140	0·00005	20	39·7	36·8	..	..	..	..	..	Died 19 hours.

venene simultaneously, but separately, under the skin on different sides of the body. These large quantities were introduced by injecting about 2 c.c. into different situations; 20 c.c. of serum is quite harmless.

The rectal temperature of each rabbit was taken at the time of injection and each twelve hours subsequently. The fall in temperature caused by the poison is a good indication of the extent to which the animal is affected.

The conclusions I feel justified in drawing from the above experiments are:—

(1) That about the same quantity of anti-venene necessary to neutralise the venom *in vitro*, is capable of doing so when the former is injected into the blood-stream, and the latter subcutaneously.

(2) At least ten to twenty times this quantity is required when they are both placed simultaneously under the skin, but in different parts of the body.

That the proportion of toxin to anti-toxin necessary to neutralise the former should be approximately the same whether they be (1) mixed in a glass, or (2) the anti-toxin be injected into the blood-stream and the toxin subcutaneously, might be expected if the nature of the antagonism between them be a chemical one, and in consideration of the evidence adduced by Kanthack,* Erhlich,† Fraser,‡ Stevens, and Meyer,§ and Cherry and myself,|| I do not see that one can come to any other conclusion.

The toxin and anti-toxin of snake-poison neutralise one another when mixed together in adequate proportions, quite irrespective of the actual quantity of each. Solutions of the two can be titrated against each other just like standard solutions with the life of a rabbit as an indicator, in which the error in the determination of the "end-point" is one fatal dose.

If anti-venene be introduced into the blood-stream the anti-toxin is there ready to neutralise the toxin as it is absorbed, and, as might have been predicted, the amount found necessary by titration outside the body is just about adequate to neutralise the toxin as it makes its appearance in the blood. The experiments indicate, however, that a slightly larger proportion of anti-toxin is necessary under these circumstances, for the rabbits 9, 10, 11 lived a little longer than rabbits 15, 16, 17. This result may very well be due to delayed chemical action due to the dilution of the anti-toxin in the blood.

* Quoted by Stevens and Meyer, 'Path. Soc. Lond. Proc.,' March 1, 1898.

† 'Fortschr. der Med.,' 1897, No. 2.

‡ *Loc. cit.*

§ 'Path. Soc. Proc.,' March 1, 1898.

|| *Loc. cit.*

The much higher proportion of anti-toxin to toxin required when separately introduced under the skin seems to necessitate the inference that anti-toxin is comparatively slowly absorbed from the subcutaneous spaces. Our chemical knowledge of this poison in *Hoplocephalus* venom and of the active principle in anti-venene, together with what is known of the physiological mechanism of absorption, is quite in accordance with the view that this anti-toxin is only capable of slowly penetrating the capillary wall, whereas the venom passes through fairly rapidly. The constituent of the venom which was used in the above experiments is an albumose. It dialyses slowly, can be filtered through a film of gelatin under pressure, although it does not pass through so readily as water or bodies of simpler molecular constitution.* It is rapidly absorbed by the blood-vessels. An animal can be killed by subcutaneous injection of a large dose in a few minutes, and the result is not retarded by previous ligature of the lymphatics from the limb and the thoracic duct.†

On the other hand, Brodie‡ filtered anti-toxic serum of diphtheria through gelatin, and found that the active properties of the serum remained with the proteids on the outside of the filter. Cherry and I confirmed this result with diphtheria anti-toxin, and found the same for anti-venene,§ and I think both these anti-toxins are bodies of great molecular size comparable to proteids. The walls of the capillaries of the limbs are membranes possessed of permeabilities approximating to those of a film of gelatin, for Starling showed they were relatively although not absolutely impermeable to proteids.|| If molecular size is the obstacle to proteid absorption from subcutaneous spaces the same would apply to anti-toxins.

Calmette has made the statement that anti-venene is more rapidly absorbed than venom. He does not adduce any experimental proof for such a statement, and I cannot see that the results detailed in this paper can bear any other interpretation than that the poison with which I have been working is absorbed 10—20 times as rapidly as the active principle in anti-venomous serum.

The practical indication of this in the treatment of snake bite is to inject the serum intravenously, until the potency of the anti-venomous serum which is at the disposal of the public is greatly enhanced.

* C. J. M., 'Roy. Soc. N.S.W. Proc.,' Aug., 1896.

† C. J. M., 'Roy. Soc. N.S.W. Proc.,' July, 1895.

‡ 'Journ. of Path.,' 1897.

§ *Loc. cit.*

|| 'Journ. of Physiol.,' vol. 19, 1895-96, p. 311.

"On the Character of the Impurity found in Nitrogen Gas derived from Urea." By LORD RAYLEIGH, F.R.S. Received August 17, 1898.

It has already* been recorded that nitrogen prepared from urea by the action of sodium hypobromite or hypochlorite, is contaminated with an impurity heavier than nitrogen. The weight of pure nitrogen in the globe employed being 2·299 grams, the gas obtained with hypochlorite was 36 milligrams, or about 1½ per cent., heavier. "A test with alkaline pyrogallate appeared to prove the absence from this gas of free oxygen, and only a trace of carbon could be detected when a considerable quantity of the gas was passed over red-hot cupric oxide into solution of baryta." Most gases heavier than nitrogen are excluded from consideration by the thorough treatment with alkali to which the material in question is subjected. In view of the large amount of the impurity, and of the fact that it was removed by passage over red-hot iron, I inclined to identify it with nitrous oxide; but it appeared that there were strong chemical objections to this explanation, and so the matter was left open at that time. This summer I have returned to it; and although it is difficult to establish by direct evidence the presence of nitrous oxide, I think there can remain little doubt that this is the true explanation of the anomaly. I need scarcely say that there is here no question of argon beyond the minute traces that might be dissolved in the liquids employed.

In the present experiments hypochlorite has been employed, and the procedure has been the same as before. The generating bottle, previously exhausted, is first charged with the full quantity of hypochlorite solution, and the urea is subsequently fed in by degrees. The gas passes in succession over cold copper turnings, solid caustic soda, and phosphoric anhydride. In various experiments the excess of weight was found to be variable, from 23 to 36 milligrams. In order to identify the impurity it was desirable to have as much of it as possible, and experiments were undertaken to find out the conditions of maximum weight. A change of procedure to one in which the urea was first introduced, so that the hypochlorite would always be on the point of exhaustion, led in the wrong direction, giving an excess of but 7 milligrams. Determinations of refractivity by the apparatus,† which uses only 12 c.c. of gas, allowed the substitution of a miniature generating vessel, and showed that the refractivity (and along with it the density) was increased by a previous heating of the hypochlorite to about 140° C. Acting upon this information, arrangements were made

* Rayleigh and Ramsay, 'Phil. Trans.,' A (1895), p. 188.

† 'Roy. Soc. Proc.,' vol. 59, p. 201, 1896; vol. 60, p. 56, 1896. See also Appendix.

for a preliminary heating of the large generating vessel and its charge, with the result that the excess of weight was raised to 55 milligrams, or about $2\frac{1}{2}$ per cent. of the whole. In any case heat is developed during the reaction, and the heavier weights of some of the earlier trials probably resulted from a more rapid generation of gas.

In seeking to obtain evidence as to the nature of the impurity, the most important question is as to the presence or the absence of carbon. The former experiment has been more than once repeated, with the result that the baryta showed a slight clouding. Parallel experiments in which CO_2 was purposely introduced, indicated that the whole carbon in a charge of gas weighing 30 milligrams in excess was about 1 milligram. It is possible (though scarcely, I think, probable) that this carbon is not to be attributed to the gas at all, and in any case the amount appears to be too small to afford an explanation of the 30 milligrams excess of weight. If carbon be excluded, the range for conjecture is much narrowed. As to oxygen, only traces were found in most of the samples examined, whereas enormous quantities would be needed to explain the excessive weight. It should be noted, however, that the extra heavy sample, showing 55 milligrams excess, gave evidence of containing a more appreciable quantity of oxygen.

It seems difficult to suggest any other impurity than nitrous oxide which could account for the anomalous weight. Unfortunately there is no direct test for nitrous oxide, but so far as the examination has been carried, the behaviour of the gas is consistent with the view that this is the principal impurity. The gas as collected has no smell. The proportion of nitrous oxide indicated by the refractometer is nearly the same as that deduced from the weight. For example, the refractivity was observed of some of the gas which weighed 55 milligrams in excess. The proportion by volume (x) of N_2O in the whole required to explain the excess of weight is given by

$$x \times \frac{22}{14} + 1 - x = \frac{2 \cdot 299 + 0 \cdot 055}{2 \cdot 299},$$

whence $$x = 0 \cdot 042.$$

The refractivity (referred to *air* as unity) of the same gas was determined by two independent sets of observations as 1·047, 1·048; mean, 1·0475. If we assume that there are only nitrogen and nitrous oxide present, the proportion (x) of the latter can be deduced from the known refractivities ($\mu - 1$) of nitrous oxide, nitrogen, and air, which are respectively 0·0005159, 0·0002977, 0·0002927, the number for air being *less* than for nitrogen. Thus,

$$x \times 5159 + (1 - x) \times 2977 = 1 \cdot 0475 \times 2927,$$

giving $$x = 0 \cdot 0408.$$

The slight want of agreement can be explained by the presence of a

little oxygen, the recognition of which would lead to a rise in the second value of x, and a fall in the first. Examination of the gas from the refractometer with alkaline pyrogallate proved that oxygen was actually present.

Evidence may also be obtained by exploding the gas with excess of hydrogen, for which purpose oxy-hydrogen gas must be added. But when nitrous oxide is in question, operations over water are useless, while for the more exact procedure with mercury, experience and appliances were somewhat deficient. The contraction observed was rather in excess of the volume of nitrous oxide supposed to be present, but of this a good part is readily explained by a small proportion of free oxygen.

If the impurity is really nitrous oxide, it should admit of concentration by solution in water. To test this, about 1 litre of water (cooled with ice) was shaken with the contents of a globe (about 2 litres). The dissolved gases were then expelled by boiling, and were collected over water rendered alkaline, in order to guard against the introduction of CO_2. The quantity was, of course, too small for weighing, but it could readily be examined in the refractometer. Of one sample, after desiccation, the refractivity relatively to air was found to be as high as 1·207, although some air was known to have entered accidentally. The proportion of nitrous oxide in a mixture with nitrogen which would have this refractivity is 0·255. The impurity thus agrees with nitrous oxide in being very much more soluble in water than are the gases of the atmosphere.

In the analytical use of hypobromite for the determination of urea, it has been noticed* that the nitrogen collected is deficient by about 8 per cent., but the matter does not appear to have been further examined. The deficiency might be attributed to a part of the urea remaining undecomposed, but more probably to oxidation of nitrogen. In default of analysis any nitrogen collected as nitrous oxide would not appear anomalous, and the explanation suggested requires the formation in addition of higher oxides retained by the alkali.

There is reason to suspect that nitrogen prepared by the action of chlorine upon ammonia is also contaminated with nitrous oxide, and this is a matter of interest, for the contamination in this case cannot well be referred to a carbon compound. In two trials with distinct samples the refractivities were decidedly in excess of that of pure nitrogen.

APPENDIX.

Details of Refractometer.

Determinations of refractivity have proved so useful and can be made so readily and upon such small quantities of gas, that it may be

* Russell and West, 'Chem. Soc. Journ.,' vol. 12, p. 749, 1874.

desirable to give further details of the apparatus employed, referring for explanation of the principles involved to the former communication already cited.

The optical parts, other than the tubes containing the gases, are mounted independently of everything else upon a bar of T-iron 90 cm. in length over all. The telescopes are cheap instruments, of about 3 cm. aperture and 30 cm. focus, from which the eye-pieces are removed. At one end of the T-iron and in the focus of the collimating telescope the original slit is fixed. This requires to be rather narrow, and was made by scraping a fine line upon a piece of silvered glass. At the further end the object-glass of the observing telescope carries two slits which give passage to the interfering pencils, and are situated opposite to the axes of the tubes holding the gases. The sole eye-piece is a short length of glass rod—the same as formerly described —of about 4 mm. diameter, which serves as horizontal magnifier. The gas tubes are of brass, about 20 cm. long and 6 mm. in bore. These are soldered together side by side and are closed at the ends by plates of worked glass, so cemented as to obstruct as little as possible the passage of light immediately over the tubes. There are two systems of bands, one formed by light which has traversed the gases within the tubes, the other by light which passes independently above; and an observation consists in so adjusting the pressures within the tubes that the two systems fit one another. Unless some further provision be made, there is necessarily a dark interval between the two systems of bands corresponding to the thickness of the walls of the tubes and any projecting cement. It is, perhaps, an improvement to bring the two sets of bands into closer juxta-position. The interval can be abolished with the aid of a bi-plate (fig. 1), formed of worked glass 4 or 5 mm. thick.* This is placed immediately in front of the object-glass of the observing telescope, the plane of junction of the two glasses being horizontal and at the level of the obstacles which are to be blotted out of the field of view.

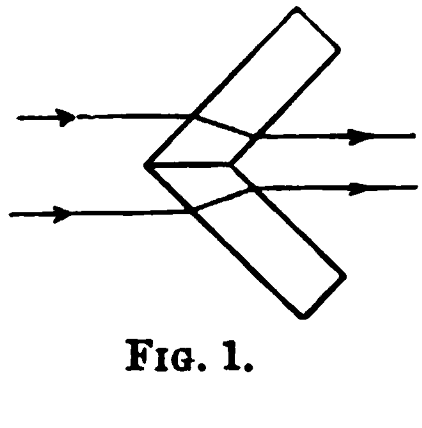


FIG. 1.

The objects sought in the design of the remainder of the apparatus were (i) the use of a minimum of gas, and (ii) independence of other pumping appliances. To this end the glass tubes associated with each optical tube were arranged so as to serve both as manometer tubes and

* *Compare Mascart*, 'Traité d'Optique,' vol. 1, p. 495, 1889.

as a sort of Geissler pump. The two halves of the apparatus being independent and similar, it will suffice to speak of that which contained the gas to be investigated. The tubes in which the levels of mercury are observed are about 1 cm. in diameter. The fixed one, corresponding to the "pump-head" of a Geissler or Töpler, is 33 cm. in length, and is surmounted by a three-way tap, allowing it to be placed in communication either with the optical tube or with one of narrow bore ending in a U, drowned in a deep mercury trough. The bottom of the fixed tube, prolonged by 92 cm. of narrower bore, is connected through a hose of black rubber with the movable manometer tube. The latter is 70 cm. long and of one bore (1 cm.) throughout. It can either be held in the hand or placed in a groove (parallel to the fixed tube) along which it can slide. The four columns of mercury stand side by side, and the levels are referred by a cathetometer to a metre scale which occupies the central position. It is not proposed to describe the cathetometer in detail, but it may be mentioned that it is of home construction, and is mounted on centres attached to the floor and ceiling of the room. It sufficed to record the levels to tenths of millimetres. The whole apparatus was constructed by Mr. Gordon.

If the glasses closing the optical tubes were perfect, there would be coincidence of bands corresponding to complete exhaustion of both optical tubes. A correction could be made for the residual error once for all determined, but it is safer to make two independent settings, one at pressures as nearly atmospheric as the case admits, and a second at minimum pressures. There are then in all eight readings to be combined. An example may be taken from a case already referred to:—

I.	II.	III.	IV.
9770	9371	9749	9790
7272	2165	2469	7445

Columns I, II refer to the anomalous nitrogen, III and IV to the dried air used as a standard of comparison. I and IV are the fixed manometer tubes in communication with the optical tubes. The reduction may be effected by subtraction of the rows:—

2498	7206	7280	2345

Thus 4708, the difference between II and I, of the nitrogen balances 4935, the difference between III and IV, of air. The refractivity referred to air is accordingly $\frac{4935}{4708}$, or 1·048.

In this example the range of pressures for the air is 493·5 mm., or about two-thirds of an atmosphere.

Great care is sometimes required to ensure matching the same bands in the two settings. A mistake of one band in the above example would entail nearly 2 per cent. error in the final result, inasmuch as the whole

number of bands concerned is about 96 per atmosphere of air, or about 62 over the range actually used. It is wise always to include a match with pressures about midway between the extremes. If the results harmonise, an error of a single band is excluded, and it is hardly possible to make a mistake of two bands.

As regards accuracy, independent final results usually agree to one-thousandth part.

"On Nagana, or Tsetse Fly Disease. (Report, made to the Tsetse Fly Committee of the Royal Society, of Observations and Experiments carried out from November, 1896, to August, 1898.)" By A. A. KANTHACK, H. E. DURHAM, and W. F. H. BLANDFORD. Received October 27, 1898.

At the request of the Colonial Office, the Royal Society of London appointed a Committee to co-operate with Surgeon-Major Bruce in his research upon Nagana or the Tsetse Fly disease. This Committee entrusted us with the actual experimental work. The object was to study Nagana systematically in ordinary laboratory animals, to investigate the life-history of the hæmatozoon discovered by Bruce, and, if possible, to discover methods of prevention, cure, or immunisation.

The material for our observations was obtained in the first instance from the blood of a dog infected by the disease on the voyage from Africa, and brought to England in November, 1896, by Dr. Waghorn.

The investigation was begun at once at the pathological laboratory of St. Bartholomew's Hospital, but in February, 1897, was transferred to the pathological laboratory of the University of Cambridge.

The Hæmatozoon of nagana has already been described by Bruce, and is closely allied to the *Trypanosoma* of Surra. We have had no opportunity of studying the latter disease, the relation of which to nagana is referred to later. The parasite discovered and described by Rouget* in a horse in Algeria is also similar. In English sewer rats (*Mus decumanus*) a *Trypanosoma* (*T. sanguinis*) is occasionally found, but this is quite distinct from the hæmatozoon of nagana, both in its morphological appearance and in its pathogenic effects (*vide infra*).

I. *Susceptibility.*

Cats, dogs, mice, rabbits, rats, both sewer rats (*Mus decumanus*) and white and piebald rats (*Mus rattus*), are highly susceptible, and in these animals the disease has proved fatal in every case of infection which has been allowed to run to a close.

* 'Annales de l'Institut Pasteur,' 1896, p. 716.

A single *hedgehog* inoculated was readily infected and died in seventeen days, so that this animal probably possesses a high susceptibility.

A single *donkey* was inoculated and was killed twelve weeks later, being then in a weak condition and near dying.

Two *horses* have been inoculated, one a strong and well-fed cart horse ("Russian"), which survived seven weeks, the other a rather old animal (see under zebra hybrids) which survived only eight days.

A *bosch-bok* has also been inoculated; it died seven months afterwards without showing any lesions. All the inoculations made from it proved negative.

Two hybrids of zebra and horse (♂ zebra and ♀ horse, and ♂ horse and ♀ zebra) and one hybrid of zebra and ass (ass ♂ and ♀ zebra) have also been inoculated. These were kindly put at the disposal of the Royal Society by Professor Cossar Ewart, of Edinburgh, in order to see whether such hybrids are refractory to nagana.

The two former were infected by plunging a needle wetted with nagana blood beneath the skin; the latter received a dose of 1 cubic centimetre of the same blood. All of them died in about eight weeks. During the course of the disease they showed irregular rises of temperature, sometimes up to 41·6° C. Variations in the number of hæmatozoa were ascertained in the case of the horse hybrids; on some occasions they were abundant (66,000 per cubic millimetre). Whenever the donkey hybrid was examined at the earlier stage of the illness the hæmatozoa were found to be either scanty or absent. A horse which was inoculated as a control died in eight days, with very abundant hæmatozoa in its blood; this animal must have been peculiarly susceptible to nagana, as no other cause for death could be found at *post-mortem* examination. There is no reason for supposing that the hybrids exhibited any more refractoriness than other horses or asses.

Koch* reports on attempts which he made to infect two Masai donkeys, and two crosses from Muscat and Masai donkeys. None of these showed any symptoms of the disease up to three and a half months, nor were hæmatozoa discovered in their blood at any time, although repeated examinations were made. Consequently there is no proof that these animals were really infected. In our experience scratch inoculations sometimes, though rarely, fail; on the other hand, inoculations by puncture with a needle or by actual injection do not fail; Koch's animals were inoculated by the scratch method. It should be added that all his control infections were successful. He did not find that ordinary mules showed any immunity.

With regard to *guinea-pigs*, at first we thought that they were refractory under normal conditions, and that it was possible to infect them only after their resistance had been reduced by bleeding or other interferences.

* 'Reiseberichte,' pp. 69 and 88.

We found, however, that guinea-pigs are susceptible to nagana under ordinary conditions, but that, as a rule, the disease in them is more protracted than in rabbits, rats, mice, cats or dogs, and even horses; so that they are distinctly more resistant than these animals. In no instance, however, has recovery ensued after hæmatozoa have once appeared in the blood.

According to unpublished observations by Bruce upon the Tsetse Fly Disease in South Africa, it appears that native *goats* and *sheep* are to some extent refractory, the disease, as a rule, running a chronic course (five months).

A *monkey* (*Macacus rhesus*) was also tried. It died in about two weeks in an advanced condition of pulmonary tuberculosis, but the presence of abundant hæmatozoa had been determined in the blood during life up to the time of death.

A *weasel* was injected. It showed hæmatozoa in its blood, and died a few days later, but death almost certainly was hastened by the effects of captivity.

Pigeons are the only *birds* which have yet been tried. The pigeons after inoculation did not show signs of the disease, nor was their blood infective. It may be mentioned that Bruce tried South African *hens* without success. Further experiments with birds are in hand.

Young animals, if susceptible (kittens and puppies), as a rule have died earlier than adults, and while suckling they are still more highly predisposed; young guinea-pigs, however, are comparable to older ones in their resistance.

The *fœtus in utero* of infected rabbits, guinea-pigs or rats, is not infected, although the mother's blood may contain a large number of hæmatozoa. The latter are to be found in the placenta, but not in the fœtal blood. Similar observations have also been made by Lewis,* Lingard,† and Rouget‡ in their investigations on allied hæmatozoa.

II. *Duration of Disease in the different Animals.*

As will be seen from the figures given below, the lethal period varies somewhat in each species of susceptible animal. The duration of the disease appears to depend principally upon the individual susceptibility rather than on the mode of inoculation or the quantity of infective material introduced. Thus, of four rabbits inoculated in the same manner with the same material, three died on the 12th, 21st, and 24th days respectively, whilst the fourth was killed on the 41st day; many similar instances could be cited. Nor does a larger quantity necessarily determine a more rapid death; thus a rabbit which has

* 'Physiol. and Pathol. Researches,' p. 630.

† 'Summary of Further Report on Surra,' 1895.

‡ '*Annales* de l'Institut Pasteur,' 1896, p. 716.

received the whole blood of another rabbit containing numerous hæmatozoa may survive longer than the minimal lethal period for rabbits. We have not been able to define the conditions which determine these variations in susceptibility. Rouget has noted similar variations in the lethal period.

The ratio of the minimal to the maximal lethal periods is about 1 to 5 or 1 to 6 in rabbits and rats, and 1 to 9 in guinea-pigs. The number of other animals inoculated has not been sufficiently great to determine a satisfactory ratio.

In our experiments dogs survived an infection 14—26 days, cats 22—26 days, rats 6—26 days, mice 8—25 days, rabbits 13—58 days, and guinea-pigs 20—183 days; the average duration being for dogs 18 days, for cats 24 days, for rats 12 days, for mice 13 days, for rabbits 30 days, and for guinea-pigs 50 days.

Since the commencement of these experiments, a large number of animals have been dealt with, and thus an extensive series of cross-inoculations has been carried out; but we have found that the duration of the disease is not dependent on the kind of animal from which the hæmatozoa are derived. No constant modification is, therefore, effected by passages, either in the direction of attenuation or of increased virulence. This statement is completely borne out by Bruce's observations on wild animals, as well as African sheep and goats, for he found that the hæmatozoa of these animals were as infective as those obtained from highly susceptible animals, such as dogs.

III. *Mode of Inoculation.*

Inoculations have been made with the blood of an infected animal, subcutaneously, intravenously, or intraperitoneally, or by applying a minute and often minimal quantity of infected blood to a superficial scratch. Rabbits have also been inoculated in the anterior chamber of the eye, and rats directly into a lymphatic gland.

Blood taken from diseased animals, although showing no hæmatozoa when examined microscopically, has frequently been proved to be fully infective, so that it appears that a single hæmatozoon, or at any rate a very small number of them, successfully introduced, are capable of producing the disease. At present no method of graduating the dose appears possible, since a minute quantity is as effective as much larger quantities, though the lethal period may be somewhat prolonged. It is also possible that unrecognised forms are present in these cases, though it should be added that in some instances where no hæmatozoa are found in simple films, we were able to detect them by means of centrifugalising the blood.

Successful inoculations have also been made with lymphatic gland, spleen, bone-marrow, aqueous humour, serous fluid, œdema transudation, and testicular juice.

The incubation period and the duration of the disease, as already pointed out, are not entirely dependent upon the number of hæmatozoa in the material injected, or the source of the infective material. Thus the hæmatozoa of lymphatic glands, &c., are as infective as those of the blood. The duration of the disease is not materially affected by the mode of inoculation adopted, and is about the same, whether the infection was brought about by subcutaneous, intravenous, or intraperitoneal injection, or by a superficial scratch.

Material taken from the bodies of animals twenty-four hours, or sometimes less, after death, is hardly ever infective, even when several cubic centimetres are injected, so that we have no evidence of a resisting or sporing form which survives in the tissues or blood of the dead animal, and is inoculable into other mammals. It must be added that putrefactive changes often set in with great rapidity in the bodies of animals dead of nagana.

Blood drawn from the living infected animal and kept *in vitro* in an aseptic condition, retains its infective power *at most* for three or four days, but this period is generally less. Complete drying also renders blood non-infective.

Blood heated to 50° for thirty minutes invariably becomes non-infective, even in large doses (such as 4 c.c.), while when heated to 46° C. for half an hour it proved infective in one out of two cases, although apparently the hæmatozoa had become non-motile—at least no motile forms were detected under the microscope. But even in this case the lethal period was not prolonged.

Infection by feeding has been attempted by means of a number of experiments. Sometimes it was successful, in most cases unsuccessful, so that it has seemed to us that the possibility of infection by the mouth depends on accidental lesions about the mouth, nose, ears (in rats), or alimentary tract.

Of a number of rats fed on organs of nagana animals, only a few acquired the disease, and these invariably showed superficial lesions of the snout and ears, due to lice. When fed upon infective material, they bury their snouts in it as well as scratch their ears with their blood-stained forepaws. Furthermore, in the rats which acquired the disease through feeding, the cervical glands were always enlarged most, which proves that the hæmatozoal infection must have taken place in the head, for, as we shall show, the primary infection travels by the lymphatics.

A cat fed repeatedly on soft tissues of the bodies of infected dogs and cats, and subsequently on the bodies of dead rats, died at a time corresponding by lethal period to an infection at the first meal on rats. We regard it as probable that some splinter of bone caused a superficial lesion through which the hæmatozoa were enabled to enter.

One rabbit, fed carefully by means of a pipette with large quantities

of infected blood, never showed the slightest sign of the disease. Rouget (*op. cit.*) also failed to infect animals by the mouth.

Two rabbits, into whose conjunctival sacs several drops of blood containing very abundant hæmatozoa, and a third rabbit whose eye was brought into contact with one of these, did not become infected. We presume that Rouget's positive results by this method were due to some accidental lesion.

A dog suffering from the disease did not infect her puppies during the last fourteen days of her life, nor did these puppies infect their foster mother (she-cat) after they had been inoculated.

Nor have we observed transmission of the disease through the mother's milk in guinea-pigs. Rouget alludes to a doubtful instance of infection by coitus in rabbits by means of the spermatic fluid. We have not detected hæmatozoa in spermatic fluid obtained from the vesiculæ seminales, and believe that in Rouget's single positive case there may have been direct infection from the penis, which suffers considerably in rabbits and may become excoriated, so that it easily bleeds.

We therefore do not believe that it is possible to infect an animal by feeding in the absence of superficial lesions, and in this respect we differ from Bruce, who seems to imply that the hæmatozoa can pass through the unbroken surface of the alimentary tract.

IV. *Symptoms and Course of the Disease.*

These vary somewhat according to the nature of the animal, but there are certain striking symptoms which commonly occur in different groups of animals. These may therefore be regarded as the most characteristic.

1. *Muscular wasting* and *loss of power* are evident in all but the small animals. In rats, mice, and guinea-pigs they are but little marked or absent altogether. In the horse, dog, cat, and rabbit the wasting is very conspicuous. In the cat, dog, rabbit, and hedgehog there is marked loss of weight, amounting to 20—30 per cent.

2. *Fever.*—In most animals which have been examined, there is a smart rise of temperature about the time of appearance of hæmatozoa in the blood. (Horse, 41·5° C.; dog, 40° C.; rabbit, 41° C.; guinea-pig, not constant.)

Paroxysms of fever are common in the *horse*, as has already been shown by Bruce. The temperature may rise to a considerable height (41·6° C.); the same is true of the zebra-horse hybrids. In a horse upon which daily observations were made, quick and sudden rises of temperature immediately followed an increase of the hæmatozoa in the blood. At the time of death there was marked pyrexia.

In the single *donkey* which we examined the temperature was generally raised throughout the course of the disease.

In *dogs* there is also fever, the temperature becoming subnormal on approach of death.

In *rabbits* pyrexia is common, and generally the temperature is elevated throughout the disease, but it may fall suddenly to normal. The temperature curve is always irregular, and no relation between the temperature curve and the hæmatozoal curve could be established.

In *cats* also the fever is well marked, the temperature falling quickly towards the end.

It is difficult to speak with certainty of the temperature in such small animals as *rats* and *mice*.

In *guinea-pigs*, representing less susceptible animals, fever as a rule is not a special feature. The temperature is as irregular as it is in the normal animal, but the animal shows paroxysmal rises from time to time, sometimes above 41°; these may be accompanied by an accession of hæmatozoa into the circulation, but this is not a constant feature.

3. *Œdema* is common in certain animals, such as the horse, rabbit, cat, and dog, and is most marked about the head, legs, belly, or genitals. In smaller animals, such as rats and mice, it is not usual, and in guinea-pigs it has not been observed. In dense tissues, as the rabbit's ear, there may be a local œdema at the site of inoculation.

Rabbits exhibit a special tendency to œdema of the external genital organs. There is often great and progressive swelling of the prepuce or labia, as the case may be. The swollen parts often excoriate and become sore and covered by crusts, so that the animal is in a sorry condition.

4. *Changes in the Eyes and Nose.*—In cats, dogs, rats and rabbits turbidity of the aqueous humour, fibrinous plaques in the anterior chamber, and corneal opacities are occasionally observed. In rabbits a muco-purulent conjunctivitis is common, and this may be followed by an opacity of the cornea and a turbidity of the aqueous humour, which under such conditions shows hæmatozoa microscopically as well as leucocytes. Hæmatozoa have also been discovered in the conjunctival discharge in the earlier stages of the disease. Vascular corneal ulcers sometimes occur in dogs, and the conjunctivitis of cats, dogs, rats and rabbits is frequently associated with œdema of the eyelids and face. In rabbits the eyelids and nose frequently become almost entirely closed up by the drying of the secretion; in the latter case they breathe with great difficulty, keeping their mouths open. This condition has been described by Rouget (*op. cit.*).

5. *Anæmia.*—Some degree of anæmia is always present, but it does not seem to be so extreme as to be the sole attributable cause of death, and points rather to a disturbance in the hæmatopoietic or the hæmatolytic mechanisms.

The number of red blood corpuscles steadily diminishes and nu-

cleated red corpuscles (normoblasts) often appear, especially in rats. According to observations on rabbits the diminution of hæmoglobin is roughly proportional to that of the blood corpuscles.

Leucocytosis is not a constant feature and when present is apparently due to the febrile temperature. An excessive leucocytosis, such as occurs in leukæmia, was never observed; 15,000—34,000 leucocytes being the highest numbers recorded per cubic millimetre.

Blood drawn from an animal seriously ill, when clotted, generally exhibits a marked buffy coat; the serum is often turbid and may undergo secondary clotting.

Instead of forming rouleaux, the red corpuscles tend to clump into masses and to lose their outlines, especially when the anæmia is pronounced (rabbit, ass, and horse).

The serum of such blood, when mixed with normal blood of the same species of animal, causes the red corpuscles to clump together also.

The urine of infected dogs spectroscopically examined often shows an intense *urobilin* band.

6. Wounds do not heal well, and tend to break down and become septic, even though the operation has been performed with strict aseptic and antiseptic precautions. The hæmatozoa may be abundant in the discharge from the wounds. Many animals, especially dogs, are apt to become infected with pyococci and other bacteria in the later stages of the disease, even when the inoculated material has been proved to be free from bacteria. We conclude that a spontaneous terminal bacterial infection may occur when the marasmus has reached a certain degree. This may accelerate death and is probably fairly often the case in the naturally acquired disease. But we have often proved by cultures that bacteria are absent in uncomplicated cases of experimental inoculation.

7. A voracious appetite has not been observed in the infected laboratory animals; some animals refuse their food and the stomach is not seldom empty after death.

8. Rats and guinea-pigs often exhibit convulsive or eclamptic seizures shortly before death, but otherwise guinea-pigs, rats, and mice show no symptoms of disease, except dulness in the later stages.

9. Transmission from one animal to another, without direct inoculation, has never been observed. Nor have we come across instances of infection by coitus or through suckling, although we have dealt with large numbers of animals.

V. *Morbid Anatomy.*

In *rats* and *mice* exactly the same conditions may be observed. The most striking changes are:—

(1) Enlargement of the lymphatic glands, the glands corresponding

to the seat of inoculation being always largest. This observation is important, because from the relative size of the glands it is possible to determine the seat of infection. To this allusion has already been made, when the effects of feeding were discussed. The glands are generally red, congested, juicy, and œdematous; in a few instances hæmorrhagic extravasation has been observed. In some cases all lymphatic glands in the body are enlarged, in others a particular series only. If a rat be inoculated in the right thigh, the glands in the left axilla and left groin suffer last.

(2) The spleen is much enlarged, with but few exceptions, and it is generally firm, friable, and dark coloured.

(3) The liver generally shows some enlargement and may be fatty.

(4) Wasting of the muscles and atrophy of the fat is, as a rule, not well marked.

(5) Sub-pleural ecchymoses are sometimes present in the lungs, accompanied by a small amount of pleural fluid.

In *rabbits* the general enlargement of lymphatic glands is less noticeable. The spleen is generally enlarged. Petechial ecchymoses are rare. Fatty degeneration of the liver is always present, and muscular wasting is often extreme. Enlargement of testes has been observed.

In *dogs* muscular wasting is well marked, the animal being often reduced to a skeleton, but the fatty tissues are generally not much affected, except at the base of the heart, where the fat may undergo œdematous degeneration. The general enlargement of the lymphatic gland is well marked, and, as in the rat, the glands are œdematous and congested, yellowish, or even show hæmorrhagic extravasations.

The spleen is also greatly enlarged, granular, firm and friable.

Pericardial effusion is common, pleural effusion may be present.

Sub-pericardial petechiæ and hæmorrhages occur frequently, sub-peritoneal occasionally, and sometimes also sub-mucous in the intestines and stomach.

In *cats* wasting is pronounced, the glands are greatly enlarged, the spleen is also enlarged, the liver is slightly enlarged. Hæmorrhages beneath the pleura and pericardium have been noticed.

In *guinea-pigs*, which clinically often show no changes or symptoms at all, the morbid changes after death are not very well marked. The spleen is generally moderately enlarged, and occasionally even considerably; it is often very soft and rather pale. The lymphatic glands are distinctly, but as a rule only slightly, enlarged, those corresponding to the seat of inoculation being always the most affected.

Hæmorrhages have been observed in the lungs and in the stomach; serous effusions and œdema have not been noted.

In all these animals the bone-marrow is sometimes dark red in colour, at other times natural, or paler than it should be. In the shafts of the long bones the fat disappears and becomes replaced by "red" marrow.

In many cases an iron reaction has been obtained with the liver, spleen, and kidney (ammonium sulphide; and $K_4FeCy_6 + HCl$).

VI. *Distribution of Hæmatozoa.*

A. *Blood.*

After a latent period of some days, hæmatozoa are invariably found in the blood at some time or other during the course of the illness.

1. *Rats.*—When the animal is inoculated with small quantities of infective blood, the latent period averages 3—4 days. When, however, a large number of hæmatozoa is inoculated into the peritoneal cavity, the parasites may be found in the blood even after a few hours.

When the hæmatozoa have once appeared in the blood, they are generally found therein to the end, gradually increasing in number till the blood literally teems with them. During the early stages of the disease, however, variations are frequently noted, inasmuch as an increase on one day may be followed by a marked decrease on the next. In a few cases they have even temporarily disappeared from the circulation for a day or two, but this is distinctly rare in rats and mice, although common in other animals.

At the later stages the hæmatozoa may amount to 2,000,000—3,000,000 per cubic millimetre.

2. *Mice.*—What has been said of rats applies also to mice.

3. *Rabbits.*—In these animals, after inoculations with minute quantities of blood, the parasites first appear in the blood in about eight days, about the same time as the pyrexial attack. They remain in the general circulation for a day or two in small numbers; this is followed by a disappearance and reappearance for a variable number of days at irregular intervals. In the animals which have been systematically examined the hæmatozoa do not appear abundantly until towards the close of the disease; the largest number which has been estimated near the time of death has been 60,000 per cubic millimetre (compare rats and mice), but even at that time they may be scanty and difficult to find. They are also to be found in the fluid of the local œdema and discharge from wounds, conjunctiva, or genitals. Although hæmatozoa may be so scanty that they cannot be discovered by the microscope (sometimes even after centrifugalising), the animals show marked clinical symptoms. Their blood has often been proved to be infective.

4. *Dogs.*—Early in the disease, from 4—6 days, the hæmatozoa may be absent from the blood, but observations on their presence during life in the lymphatic glands have not been made. Towards the end they become very numerous (100,000—300,000 per cubic millimetre). Varia tions in the number of hæmatozoa are common, but as a rule hæmatozoa are numerous throughout the disease.

5. *Cats.*—The latent period is about five days; then the hæmatozoa

appear in the blood, and, with daily variations, quickly increase in number. The variations are sometimes remarkable; thus on one day the hæmatozoa may be extremely numerous, while on the next day they will have become scanty.

6. *Horse.*—Systematic observations on this animal, as well as on the donkey, have been made by Bruce. In our first horse the latent period was seven days. The first appearance of hæmatozoa in the blood was followed by a sharp rise of the temperature. After the hæmatozoa once showed themselves they were generally scanty and often absent (the centrifuge was not used), but an appearance of the hæmatozoa in the blood was generally followed immediately by a paroxysm of fever. A few days before death, however, the number increased greatly, falling again to zero two days before death and being low at the time of death.

7. *Guinea-pigs.*—After a subcutaneous inoculation, a few hæmatozoa will generally be found in the blood about the fifth to seventh day. They may then again disappear and reappear from time to time, to disappear again after a few days. This alternation may go on for weeks. Then suddenly the hæmatozoa become numerous and gradually increase, sometimes with irregular variations, till the blood is almost crowded, 200,000—500,000 per cubic millimetre being present. The guinea-pigs die, generally without showing any symptoms, except perhaps convulsive attacks a day or two before death.

In some cases no hæmatozoa have been found in the blood for over six weeks, although it has been examined daily. They then appeared in small numbers, and after remaining scarce for a week or so, suddenly and rapidly increased as the disease approached its fatal termination. It is, however, more common to find a few hæmatozoa about a week after inoculation, this being followed by a more or less prolonged period of absence.

In cases where the disease runs a less protracted course, the hæmatozoa become numerous about four weeks after the inoculation, when they are often present in large numbers; but, as in the case of other animals, the number of hæmatozoa may be very variable, being almost enormous one day and very considerably less, or even very small, the next. In a case where the guinea-pig had been bled before inoculation the disease ran a rather short course; hæmatozoa appeared nine days after the infection, rapidly rising in number to over 128,000 per cubic millimetre, the animal dying after twenty-two days. In a few cases where the lymphatic gland corresponding to the seat of inoculation was examined, hæmatozoa were found in the gland whilst they were absent in the blood.

B. *Lymphatic Glands.*

In the rat the superficial lymphatic glands may be readily examined by piercing them with fine capillaries or sharp needles; they may also be excised and examined more thoroughly. Although a considerable number of observations have been made by these means, and also after killing the animals at various periods after inoculation, we wish to speak somewhat guardedly, since the appearances are not quite constant. Moreover, we are at present unable to be certain that unrecognised developmental forms have not been overlooked.

By the study of the right inguinal glands after subcutaneous inoculation in the right thigh, we find that the hæmatozoa are present from one to three days before they are discoverable in the blood (taken from the ear or right leg). Again, they may be very abundant in the gland when they are still scanty in the blood. Moreover, the number in the blood may increase, whilst that in the gland decreases. In these earlier stages the hæmatozoa may be extremely numerous, forming tangles and clusters in the lymph gland, whilst only a few scattered ones are to be found in the blood. The first appearance of hæmatozoa in the gland of the other side is apparently associated with their appearance in the blood.

The observations, fewer in number, which have been made upon guinea-pigs, also point to a multiplication in or about the nearest chain of lymphatic glands in the first instance.

We have not yet determined whether these hæmatozoa pass directly into the blood through the local blood vessels, or whether they are distributed by means of lymphatic paths into the main circulation.

The animals may appear comparatively well whilst large numbers of parasites are present in their blood and glands, this is especially the case with rats and guinea-pigs. On the other hand they may be seriously ill whilst the hæmatozoa are scanty in their blood; this obtains usually in rabbits, in which animals, as already stated, the glands do not become so much enlarged, and it is possible that the main effect of the parasites is borne by other organs. For instance, at times the bone-marrow has shown the presence of hæmatozoa, although search in other organs and in the blood proved negative.

After death in the various animals, hæmatozoa are to be found in most cases in the bone-marrow and spleen. The adult hæmatozoa may be common in these situations when but few are present in the blood; but this is not constant, for the reverse may be the case. Multiplication of the parasites certainly takes place in the lymphatic glands (rat) as well as in the infected area of connective tissue; it may also occur in the above-mentioned organs as well, and perhaps too in the blood, but of this we have no certain evidence.

The hæmatozoa are also found in the fluids of the serous cavities, at any rate when they are present also in the blood.

They have not been found in the intestinal contents, nor have they been seen in the urine, except in one case in which hæmaturia and submucous petechiæ of the bladder were present (rat).

It is evident from these observations that, in order to investigate the development of the hæmatozoa in the rat, &c., special attention must be paid to the seat of inoculation and the nearest lymphatic glands.

Dead and non-motile forms may frequently be found in the circulation and in the lymphatic glands, when the disease is advanced. These are less defined and are ghost-like, being somewhat swollen in appearance; they are also generally in an extended condition.

VII. *Toxic Power of the Blood.*

The fact that animals may appear to be well for days while hæmatozoa are abundant in their blood, suggests that the hæmatozoa do not secrete much, if any, specific toxin, and indeed so far no direct evidence has been obtained of a potent poison manufactured by the hæmatozoa, either by secretion or by chemical changes induced in the blood.

Fresh serum after filtration through Berkefeld filters, and blood or serum which had been kept for days in a sterile condition till the hæmatozoa had died, have had no specific toxic effects, even when large quantities have been injected into dogs, rats, or rabbits. Blood in which the hæmatozoa have been killed by exposure to 50° C. has had no more effect. The extracts of organs obtained from diseased animals have also shown no poisonous properties.

The whole available blood of highly diseased rabbits has been injected immediately after removal into healthy rabbits, without producing immediate symptoms of acute intoxication.

The bile of diseased animals does not appear to be more toxic than that of healthy animals.

A cat, into the peritoneal cavity of which a collodion sac full of fresh infected blood had been inserted, showed no signs of illness. It was fully susceptible on subsequent inoculation.

Dogs, when injected with large quantities of filtered serum from an infected dog, showed no symptoms of a profound toxæmia.

Our experiments do not point to the presence of any intense specific toxin or poison in the blood.

VIII. *Immunisation and Cure.*

The endeavours to produce immunity, or to cure the disease after its establishment, are shortly summed up as follows:—

1. Animals which have been repeatedly injected with blood or serum of nagana animals, such blood or serum having been previously freed from living hæmatozoa, either by filtration, heat or by allowing it to stand for a week or longer, have not shown the slightest degree of an acquired immunity. Rats, rabbits, and dogs have been tested in this manner, but none of these animals have shown any diminution in susceptibility.

2. Animals repeatedly injected with extracts of the organs of diseased animals have acquired no resistance.

3. The blood of almost full-term fœtuses, prematurely born of highly diseased rabbits, has been tried, but without the slightest success in prevention or cure.

4. The guinea-pig being a comparatively resistant animal, its serum has been used, but it also has no immunising action.

5. Repeated inoculations of bile of diseased animals have been without preventive or curative effects, although *in vitro* bile, which is always free from hæmatozoa, rapidly destroys the hæmatozoa. Infective blood mixed with sufficient bile becomes non-infective, but confers no immunity.

6. Previous inoculations with the hæmatozoon of the ordinary rat (*T. sanguinis*) have also been valueless.

7. Sewer rats and white rats which have been repeatedly, but unsuccessfully, inoculated with the ordinary rat-hæmatozoon (*T. sanguinis*), and have been proved to be refractory to further inoculations with this hæmatozoon, have all contracted nagana when subsequently inoculated, and have died in the same time as control animals treated with an equal dose of infective blood.

8. The young born of infected mothers (dog, guinea-pigs), are no more resistant than those born of normal animals.

9. As already stated, by constant transmission through different species, the nagana hæmatozoon has shown no definite loss or gain in the intensity of virulence.

10. Of immunising sera the diphtheria antitoxin and antistreptococcus serum have been used, but, as we expected, without the slightest effect: they neither protect nor cure.

11. *Dieting.*—Rats have been fed, on the one hand, exclusively with meat, and on the other, with green vegetables; in neither case has any increased resistance or prolongation of life resulted from this alteration in diet.

12. Excision of the lymphatic glands immediately after inoculation or after they have begun to show enlargement has been of no avail.

13. Feeding with hæmatozoa also conveys no immunity.

IX. *Allied Hæmatozoa.*

1. *Trypanosoma Sanguinis.*

As already mentioned, in sewer rats (*Mus decumanus*) a trypanosoma may be found in a certain percentage of individuals. This hæmatozoon is distinct from that of nagana morphologically, and also as regards its pathogenic effects. Thus (1) the *T. sanguinis* has not been communicated to the dog, cat, rabbit, or mouse, even when large quantities of blood were used for inoculation. (2) In some guinea-pigs it has been found in very small numbers in the blood for two or three consecutive days, usually from about the fifth day after injection; but there has been no persistence. (3) In white rats many unsuccessful inoculations have been made with the *T. sanguinis*, even with considerable doses, and it appears that the minimal infective dose is larger than with the nagana *Trypanosoma*. (4) White rats may lose the hæmatozoon after they have been proved to have been successfully inoculated with the *T. sanguinis*. (5) Some of those which had had, and then lost, the parasite proved refractory on re-inoculation. Black and white (piebald) rats have never been successfully inoculated in our experience. (6) No rat has been successfully inoculated with the *T. sanguinis*, except at the first attempt. (7) We have not been able to recognise any illness after successful inoculations with *T. sanguinis*. Any pathogenic effect it may have must be slight. Infected rats remained alive for months; we have not observed any instance where death was to be ascribed to the hæmatozoon.

R. Koch* examined rats in Dar-es-salam, and also recognised differences between the hæmatozoon of the local rats and the *Trypanosoma* of the Tsetse disease. Whether the *Trypanosoma* occurring in the African rats examined by him is identical with that occurring in our English rats we cannot decide, although from the brief description given by Koch they certainly closely resemble each other. Nor did Koch succeed in infecting animals other than rats with the African rat Trypanosoma. He therefore showed that the parasites which occur in the blood of rats (in Dar-es-salam) do not stand in any relation whatever to the Tsetse disease of horses and cattle. In this connexion the observations of Bruce are of great value, because they prove that in big game the Tsetse parasite certainly does occur without apparently causing acute, fatal, or even obvious disease; in the same manner, this parasite may sojourn in the body of the guinea-pig for weeks and months without interfering with the health and development of the animal for a long time. Furthermore, Bruce (in an unpublished report) has made similar observations on the South African goat and sheep; he shows that in these animals the disease runs an extremely protracted course, and lasts for months.

* 'Reiseberichte,' 1898, pp. 70 and 71.

2. *Trypanosoma of Surra* (Trypanosoma Evansi).

Koch announces that the disease known as Surra in India and the Tsetse disease of Africa are produced by the same parasite. His reasons for this assumption are apparently not based on extensive personal comparative observation.* Lingard, working in areas naturally infected with surra, has not clearly distinguished between the Trypanosoma of surra and that occurring in rats in India. His account of surra as it occurs in rabbits, and guinea-pigs, and rats is suggestive of this disease being closely similar to nagana, but it is impossible for us to pretend to give any final decision in the matter. To illustrate the confusion in which Lingard has placed the matter, it may be pointed out that he writes that cows, horses, monkeys, and field rats are susceptible to inoculation with the ordinary Indian rat hæmatozoon, but that rabbits, guinea-pigs, dogs, cats, and donkeys are insusceptible, but he adds that the latter animals are susceptible after this rat hæmatozoon has been passed through the horse. He also asserts that surra can be produced in horses by feeding on the excrement of rats. These observations are calculated to create a certain amount of suspicion, particularly when it is remembered that Vandyke Carter failed to infect horses with the Indian rat Trypanosoma.† For the present therefore the question must be left open till an opportunity arises of studying the various parasites at the same time side by side, both in their morphology and pathogeny.

3. *Trypanosoma of Rouget.*‡

Rouget describes Trypanosoma disease in Algeria, which apparently is identical with the disease described by Bruce. Judging from the drawings and descriptions, his parasite agrees with that of nagana. He will not commit himself as to the identity of his parasite with that of surra. White rats and mice, rabbits, and dogs exhibited a considerable susceptibility, while guinea-pigs, he says, were refractory (possibly because he did not recognise the chronicity of the disease). Sewer rats were not always susceptible, while some showed a relative immunity. It seems, however, that he did not re-inoculate them so as to test their immunity again. His description of the symptoms and anatomical appearances in mice, rats, rabbits, and dogs agrees almost exactly with our own observations. He claims to have succeeded in immunising a number of mice by injecting them with the serum of an infected rabbit previous to inoculating the Trypanosoma; six mice survived altogether,§

* 'Reiseberichte,' p. 66.

† 'Scientific Memoirs of Med. Officers of the Army of India,' 1887, Part III, p. 56.

‡ 'Annales de l'Institut Pasteur,' 1896, vol. 10, p. 716.

§ It does not appear whether they were again tested.

while others lived for 17—23 days. As a curative agent this serum was useless; guinea-pigs serum also had no preventive action.

X. *Biology of the Nagana Hæmatozoon.*

So far our knowledge of the nagana parasite, as that of other Trypanosomas, is very incomplete. Rouget* has failed to find forms corresponding to those described by Danilevsky in birds and by Shalashnikov in rats, and Lewis and others have been equally unsuccessful, while it is difficult to follow Lingard in his description of young forms. We have not succeeded in tracing a life history, and we are still in search of developmental forms, a task which at present occupies our special attention.

1. Most commonly in blood, &c., drawn from infected animals the forms described by Bruce are found. They are generally in active movement, and can sometimes be observed in locomotion with their flagellated end forwards, as Lewis described in the case of other hæmatozoa; in many cases they do not change their position by free swimming, but tend to fix themselves by one or other end to the coverslip or to corpuscles or cells in the specimen; they then exhibit more or less rapid oscillations, and may change their position by apparently drawing or pushing themselves in one direction or the other. Meanwhile the vibratile membrane waves rapidly and the protoplasmic body alters in shape, becoming thicker and shorter or thinner and longer; in the case of the English rat hæmatozoon free swimming is the rule; changes in the shape of the body like those of the nagana organism are not observed.

2. The nagana parasites vary considerably both in size and form; they may be long and pointed or blunt-ended and somewhat stouter; some individuals are short and thick with a short flagellum, their protoplasm being crowded with rounded granules. Still larger forms possessing more than one vibratile membrane are sometimes, though rarely, met with.

3. Especially in specimens taken from lymphatic glands, but also in specimens obtained from the blood, &c., there is a clear vacuole at the thick end; this does not become stained with staining reagents; it varies much in size in the different individuals, but we have watched in vain for evidence that it is of a contractile nature.

4. By means of hæmalum or hæmatoxylin a nuclear body can be demonstrated in the middle of the parasite; it is usually oval, but may be more saddle-shaped. The protoplasm also contains a number of granules which stain with basophil reaction (methylene-blue, thionin, &c.); these are somewhat variable in number, being fewer in specimens in which the protoplasm is more refractive, *e.g.*, from lymphatic gland.

* *Loc. cit.*, pp. 722 and 723.

These granules are distributed irregularly throughout the body of the parasite; they do not occur in the membrane or the flagellum. The chromatin spot situated close to the non-flagellated end in the *T. sanguinis* is not defined in the Nagana *Trypanosoma*. The *T. sanguinis* also does not stain at all readily even by basic aniline dyes (dahlia, fuchsin, &c.), whilst that of Tsetse disease is more readily coloured by these reagents.

5. Examples joined by the poles opposite the flagellum are common at times, but although they suggest perhaps conjugation, we have no evidence that this process does really occur. After prolonged observation no further changes have been noticed in these joined individuals.

In freshly drawn blood, or in the lymph of lymphatic glands, or in pleural and peritoneal fluid, when the hæmatozoa are common, tangles made up of numerous hæmatozoa have been observed; this has been already described by Lewis. The hæmatozoa often converge with their non-flagellated ends towards one common point. In lymphatic glands, before the hæmatozoa are found in the blood, such tangles may be present in great numbers.

6. Forms consisting apparently of two individuals joined side by side by their bodies, the flagella being free, have been observed on rare occasions; from prolonged observation in the living state we have no reason to suppose that these are undergoing longitudinal fission. Especially in kept blood, &c., many of the hæmatozoa may present a rhomboid outline whilst still motile.

In drawn blood or serous fluids the hæmatozoa eventually become motionless; this may occur rapidly, for instance in twenty minutes, but generally some motile specimens can be found after two to three days—sometimes, indeed, after as long as five or six. When they are abundant, the tangles above noted are formed. Then the bodies of the organisms become rounded, the nuclear bodies becoming more distinct and readily stainable (hæmatoxylin or basic aniline dyes). At the same time the vibratile membrane or fin and the flagellum separate, forming a rather rigid filament. Eventually (generally after three to four days) masses of spherules alone remain; these apparently correspond to the nuclear bodies. In numerous experiments these structures have uniformly proved to be non-infective, and it must therefore be inferred that if they are not simply degeneration products, they require other conditions for their further development than those that are found in warm-blooded animals. All individuals, however, do not pass through these changes; some, or even all, may simply become non-motile, stiff, and pale, at the same time retaining their form. In numerous attempts at cultivation in normal blood, similar phenomena are observed without any evidence of multiplication. Within a corpse, the blood and organs become non-infective in about twenty-four hours, the changes in the hæmatozoa being similar to those just described.

Exposure for several hours in an atmosphere of hydrogen or CO_2 has no appreciable effect on the motility of hæmatozoa in blood-free serous fluids. The hæmatozoa are also fully active in hosts killed by ether, chloroform, or coal-gas.

7. Oval forms, smaller than the ordinary hæmatozoa, with (or without) a short flagellum, and often with a "beak" at the opposite pole, have been observed in the organs, but rarely in circulating blood.

8. Small rounded or ovoid bodies, about 1—2 μ in diameter, hyaline, sometimes with a refringent chromatin spot or bipolar spots, or irregular (? amœba-like) bodies, also sometimes with a chromatin spot and of the same size or rather larger, have been observed, especially in the lymphatic glands and bone-marrow, or spleen. It is possible that these are early stages in the development of the hæmatozoon. No forms have been seen at any time within the red blood corpuscles.

9. Neither sporocystic nor larger distinctly amœboid forms have been observed.

As our observations on the development of the parasites are still in an incomplete condition, this short statement must suffice, and a more detailed description must be left for a future occasion, when we submit a full report upon our work.

November 17, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Major MacMahon, Dr. Ludwig Mond, and Professor Seeley were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers received during the recess and published, in full or in abstract, in accordance with the Standing Orders of Council, were read in title :—

Professor A. W. RÜCKER, Sec. R.S. and W. H. WHITE. On the Determination of the Magnetic Susceptibility of Rocks.

Dr. VAUGHAN HARLEY. The Influence of Removal of the Large Intestine and increasing Quantities of Fat in the Diet on general Metabolism in Dogs.

The LORD RAYLEIGH, F.R.S. On the Character of the Impurity found in Nitrogen Gas derived from Urea.

Dr. C. J. MARTIN. Further Observations concerning the Relation of the Toxin and Anti-Toxin of Snake-Venom.

The following Papers were read :—

I. "Further Note on the Sensory Nerves of the Eye-Muscles." By Professor SHERRINGTON, F.R.S.

II. "Further Observations on the Effects of Partial Thyroidectomy." By W. EDMUNDS.

III. "Contributions to our Knowledge of the Formation, Storage, and Depletion of Carbohydrates in Monocotyledons." By J. PARKIN.

IV. "An Experiment in search of a Directive Action of one Quartz Crystal on another." By Professor POYNTING, F.R.S., and P. L. GRAY.

V. "The Electrical Conductivity and Luminosity of Flames containing Vaporised Salts." By Professor SMITHELLS, H. M. DAWSON, and H. A. WILSON.

"Further Note on the Sensory Nerves of the Eye-Muscles." By C. S. SHERRINGTON, M.A., M.D., F.R.S., University College, Liverpool. Received September 21,—Read November 17, 1898.

In a communication on the sensory nerves of muscles laid before the Society last year,* the question previously raised† as to the existence of afferent nerve fibres in the so-called "motor" cranial nerves for the muscles of the eye was advanced to the following position. Myelinated nerve-fibres from the IIIrd cranial and IVth cranial nerve-roots were traced into the tendons of the recti and oblique muscles. Since then control observations have been made. The ophthalmic division of the Vth cranial has been severed at its origin, and with it the VIth cranial trunk. This has been done in the monkey. The condition of the nerve branches going to and lying within the muscles and tendons has been examined after an interval of twelve to fourteen days. Those of the external rectus contained nothing but degenerate nerve-fibres, save for a few fine myelinate fibres, probably from the ciliary ganglion. Those of all of the other eye-muscles contained exclusively healthy nerve-fibres. The sensorial musculo-tendinous organs of the eye-muscles are, therefore, not innervated by the ophthalmic division of the trigeminus. On the other hand, the nerve-fibres of the external rectus muscle behave, after severance of the VIth cranial nerve, in the same way as my previous papers showed those of the other eye muscles do after section of the IIIrd and IVth nerves.

A contribution towards the physiological inquiry into the matter has been made as follows, and has given a clear reply. The conjunctivæ, both palpebral and ocular, and the corneæ of both eyes have been rendered deeply anæsthetic to cold, warmth, touch, and pain by liberal applications of cocaine. Then in a completely dark room the power to direct the gaze with accuracy in any required direction has been tested. The person under examination is seated, with the head securely fixed, in front of a screen. One of his hands carries a marker. The hand is moved by an assistant, and is made to mark the screen at some one point; it is then passively replaced. The person under observation during this time keeps the eyes open in the primary position or sits with them closed. He is then required to direct his gaze to the spot marked on the screen. The light is then switched on, and the point to which the gaze is turned is noted. The power to direct the gaze under these circumstances has been found to remain good. If for co-ordinate execution and ability to perform the delicate adjustments for training

* C. S. Sherrington, 'Roy. Soc. Proc.,' vol. 61, p. 247, February, 1897.

† C. S. Sherrington, 'Physiol. Soc. Proc.,' No. 3, June, 1894.

the eyeballs correctly in any desired position the exercise of peripheral apparatus of muscular sense is required, the only possible channels under the above conditions would seem to be deep branches of the Vth nerve or the IIIrd, IVth, and VIth so-called "motor" nerves themselves. As previously stated, the former are by both my earlier and later degeneration experiments excluded. The latter, therefore, are the only ones remaining, for the superficial branches of the Vth and the retinæ are put out of action by the conditions of experiment.

I am indebted to Mr. E. E. Laslett for carrying out the observations with me. Details regarding the methods employed and the results obtained will be given in a completer paper written in conjunction with him.

"An Experiment in Search of a Directive Action of one Quartz Crystal on another." By J. H. Poynting, Sc.D., F.R.S., and P. L Gray, B.Sc. Received September 27,—Read November 17, 1898.

(Abstract.)

A quartz sphere, 0·9 cm. diameter, weight 1·004 grams, was suspended by a long quartz fibre so that its time of vibration was about 120 seconds. A second quartz sphere, 6·6 cm. diameter, weighing 399·9 grams, with its centre on a level with that of the first and 5·9 cm. from it, was rotated continuously in a period of 115 seconds in one series, and in a period of 230 seconds in another series of observations.

The axis of the smaller sphere was horizontal and perpendicular to the line through the centres. Any directive action should manifest itself as a periodic couple, producing forced oscillations in the smaller sphere.

If the ends of the axis of a quartz crystal are indistinguishable the couple should go through its values in half a revolution of the larger sphere. This is termed the "quadrantal" couple, and to test for it the time of revolution was 230 seconds, or nearly double that of the suspended sphere. If the ends of the axis are poles, like those of a magnet, the couple should go through its values only in a complete revolution. This is termed the "semi-circular" couple, and to test for it the time of revolution was 115 seconds, or nearly equal to that of the suspended sphere. The position of this latter sphere was read by means of mirror and scale every 11·5 seconds, *i.e.*, at ten equidistant phases of the 115-seconds period. By taking a large number of periods, the mean reading for each phase should be freed to a great extent from other periodic motions and accidental disturbances, and a 115-second vibration should, if it existed, be rendered evident.

The quadrantal and semi-circular series of observations both gave evidence of periodic vibrations of 115 seconds, but so small that they could only be put down as giving superior limits, and not at all as proving the existence of the couples.

Assuming that the gravitation constant in the quadrantal case is G for parallel and G′ for crossed axes, the existence of a couple enables us to find $(G - G')/G$, and the observations show that this fraction is not greater than 1/16500.

Assuming that the gravitation constant in the semi-circular case is G for like parallel axes, and G′ for unlike parallel axes, $(G - G')/G$ is not greater than 1/2850. The semi-circular vibration outstanding after the elimination of disturbances was much greater than the quadrantal, no doubt owing to the fact that want of axial symmetry would itself lead to a semi-circular couple; and though an attempt was made to eliminate the effect, it was probably unsuccessful.

"Contributions to our Knowledge of the Formation, Storage, and Depletion of Carbohydrates in Monocotyledons." By JOHN PARKIN, M.A., Trin. Coll., Camb. Communicated by Professor MARSHALL WARD. Received July 16,—Read November 17, 1898.

(Abstract.)

The paper is divided into two parts, the first dealing with the formation of starch by assimilation in the leaves, the second with the occurrence of starch and inulins in the reserve-organs of various Monocotyledons.

The author has investigated about seventy species, belonging to all the principal groups of Monocotyledons, some of them at various different stages of growth, and finds that starch due to normal assimilation in the leaves occurs in very different amounts in different genera. Relatively few produce much, and some form none at all, but species from most of the principal families form some starch in their mesophyll.

On comparing the type of leaf, its position and age, the habit of the plant, and the period of normal activity, the author is led to suggest that some connection exists with the storage or non-storage of temporary starch. Broad and cauline leaves, those of aquatic Monocotyledons, and those working at higher temperatures in the summer, seem more prone to have starch than narrow radical leaves, those of forms in dry situations, and those of spring species. That the age of the leaf affects the question is shown by the results with *Allium*, a genus long known not to form starch under ordinary conditions: the

author finds that starch is developed even in this plant in the young leaves.

The starch of the stomatal guard cells is next examined, and the difficulty of depleting these cells discussed. In experiments with cut leaves exposed to sunlight little or no appreciable increase of starch could be obtained. In experiments with pieces of leaves floated on sugar solutions, cane sugar was found to produce starch far better than any other; invert-sugar, glucose, and fructose follow next in order, and maltose is almost useless.

The necessary details of the experiments, and discussion of results and previous literature are given in the full paper.

In Part II the author deals in detail with certain inulins which he has discovered in *Scilla nutans* and *Galanthus nivalis*, and shows by the examination of many other genera that inulin is by no means uncommon in Monocotyledons.

The inulin of *Scilla* is remarkable for its easy solubility in cold water, while that of *Galanthus* requires water at 80° C. for solution; ordinary inulin from *Helianthus* and other Compositæ dissolves at about 50° C.

The proofs of the inulin nature of these bodies, their reactions and mode of occurrence are worked out in detail. Contrary to previous assumption, inulin and starch may co-exist in the same cell.

It is interesting to note that aquatic species do not store inulin, apparently, but that it is common in those inhabiting dry situations; the author regards the concentrated solution in the cell-sap of such plants as useful in resisting drought.

The paper concludes with a detailed examination of the behaviour of the starch and inulin in the bulb of *Galanthus* at various periods throughout its whole annual cycle of development, comparing the stages with those in the bulb of *Narcissus*.

Summaries of the literature, and illustrations, accompany the full paper.

"Further Observations on the Effects of Partial Thyroidectomy."

By WALTER EDMUNDS. Communicated by Dr. ROSE BRADFORD, F.R.S. Received October 17,—Read November 17, 1898.

(From the Laboratory of the Brown Institution.)

Two years ago Vassale and Generali published some interesting experiments on the thyroid; they found that excision of the four parathyroids that occur in dogs (leaving the thyroid lobes) was followed by symptoms practically identical with those produced by excision of the entire thyroid, including the parathyroids.

These observations I have repeated: eighteen experiments were made in dogs; in all it was intended to excise all the parathyroids, leaving, as a rule, the thyroid proper; but in seven it was subsequently found that one, and in one case two, of the smaller parathyroids had escaped excision; these seven experiments were therefore cases of partial parathyroidectomy or parectomy.

Of the eleven experiments in which the whole of the parathyroids is believed to have been removed, in two, one of the thyroid lobes was removed at the same time; of these two, one dog died the night after operation and the other survived the operation, but died when the remaining thyroid lobe was subsequently removed.

Of the nine cases in which the parathyroids only were removed, four died: one in two days, one in four days, one in seven days, and one in twenty-eight days.

The other five survived the operation, two after temporary symptoms and three without.

Thus of the nine total parectomies, four died, two recovered after symptoms, and three without.

The five which recovered were submitted to further excision of one thyroid lobe, and (if they survived this) of the other thyroid lobe. Two died after removal of the first lobe, one died after removal of the second lobe, and two survived even this, which amounted to total thyroidectomy, but they both had temporary symptoms.

The symptoms produced by these operations were (1) tremors, (2) a slow and most unstable gait, sometimes going on to paralysis of the hind limbs, and (3) emaciation and weakness.

In two of the dogs an interesting eye symptom was noticed, viz., a narrowing of the palpebral fissures with apparent recession of the eyeballs. This occurred in one dog who succumbed to excision of the parathyroids only, and in one dog who survived this, but died after excision of one thyroid lobe.

In six of the eleven cases microscopic changes were found in the thyroid lobes left at the excision of the parathyroids; these changes were (1) a diminution or absence of the colloid in the vesicles, (2) an excessive amount of intervesicular young thyroid tissue, (3) multiplication of secreting cells in the thyroid vesicles, and (4) the secreting cells becoming columnar. These changes are very similar to or identical with those described as compensating hypertrophy of the thyroid, and to those found in Graves's disease.

In seven dogs the larger parathyroids were removed, but it was subsequently found that one, and in one case two, of the smaller parathyroids had escaped excision, these were therefore cases of partial parectomy.

Notwithstanding the incompleteness of the excision, in one of these cases the animal succumbed to the operation after seventy-two days.

The other six dogs did not die, but four of them had temporary symptoms; they were submitted to further thyroid excisions, from which they all died, one after simultaneous excision of both thyroid lobes, three after removal of one thyroid lobe only; two survived this operation, one of them having temporary symptoms; these both died after removal of the remaining lobe.

In this series, too, besides the usual symptoms of tremors, unstable gait and paralysis, emaciation and weakness, eye symptoms were observed,—in four out of the seven dogs.

In two, after the partial parectomy, the eyes became unduly prominent, but subsequently, after a further operation on the thyroid, the palpebral fissures became narrow, and the eyeballs retracted.

In one of the partial parectomies no change was noticed, but after one of the thyroid lobes had been excised the eyes became unduly prominent, and after the remaining lobe had been excised the palpebral fissures became narrowed, the eyeballs retracted, and the animal died.

In one, after removal of some of the parathyroids, and also, after an interval, of one of the thyroid lobes, no change was noticed; but after the removal of the remaining lobe with a parathyroid attached, the palpebral fissures became narrowed, and the eyes retracted.

Altogther in the eighteen dogs operated on, ocular changes were noticed in six.

In the partial parectomies, changes in the thyroid lobes left were also observed, the same as those found in complete parectomies and in Graves's disease.

Similar effects on the eyes were also noticed in monkeys, but these, eight of them, were subjected to total excision of the thyroid including the parathyroids; four of the monkeys died as a consequence of the operation: one in thirteen days, one in thirty-six days, one in sixty-eight days, and one in 262 days; three of them were treated with thyroid extract, but this, although it produced marked benefit, did not save their lives. One monkey was killed by accident; the other three are still alive.

As to their eye symptoms: in two the eyes appeared to be more prominent, and the palpebral fissures were wider; and in two the palpebral fissures were narrowed, and the eyes appeared sunken. In four no change could be seen. Drawings of the monkeys were made, before and after operation.

November 24, 1898.

Dr. W. J. RUSSELL, Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—Lord Lister, F.R.C.S., D.C.L.

Treasurer.—Alfred Bray Kempe, M.A.

Secretaries.— { Professor Michael Foster, M.A., M.D., D.C.L., LL.D.
Professor Arthur William Rücker, M.A., D.Sc.

Foreign Secretary.—Sir Edward Frankland, K.C.B., D.C.L., LL.D.

Other Members of the Council.—Professor Thomas George Bonney, D.Sc.; Captain Ettrick William Creak, R.N.; Professor Daniel John Cunningham, M.D.; Professor James Dewar, M.A.; Professor William Dobinson Halliburton, M.D.; Professor William Abbott Herdman, D.Sc.; Victor A. H. Horsley, F.R.C.S.; Joseph Larmor, D.Sc.; Professor Nevil Story Maskelyne, M.A.; Sir Andrew Noble, K.C.B.; Professor Edward Bagnall Poulton, M.A.; William James Russell, Ph.D.; Professor Arthur Schuster, Ph.D.; Dukinfield Henry Scott, M.A.; George Johnstone Stoney, D.Sc.; Professor Joseph John Thomson, M.A.

The following Papers were read:—

I. "Preliminary Note on the Spectrum of the Corona." By Sir J. NORMAN LOCKYER, K.C.B., F.R.S.

II. "On the Condensation Nuclei produced in Gases by the Action of Röntgen Rays, Uranium Rays, Ultra-violet Light, and other Agents." By C. T. R. WILSON. Communicated by Professor J. J. THOMSON, F.R.S.

III. "The Origin of the Gases evolved on Heating Mineral Substances, Meteorites, &c." By Dr. M. W. TRAVERS. Communicated by Professor RAMSAY, F.R.S.

IV. "Memoir on the Theory of the Partitions of Numbers. Part II." By Major MACMAHON, F.R.S.

"On the Condensation Nuclei produced in Gases by the Action of Röntgen Rays, Uranium Rays, Ultra-violet Light, and other Agents." By C. T. R. WILSON, M.A., Clerk-Maxwell Student in the University of Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received October 29,—Read November 24, 1898.

(Abstract.)

The experiments here described consist mainly in determinations of the least degree of supersaturation necessary to cause water vapour to condense on nuclei from various sources.

As in former experiments* the supersaturation is brought about by very sudden expansion of air or other gas originally saturated with water vapour, the expansion required to produce a fog or shower of drops being measured. The expansion is expressed in terms of v_2/v_1, the ratio of the final to the initial volume.

The following classes of nuclei have been studied in this way:—

1. Nuclei produced by Röntgen rays.
2. " " " uranium rays.
3. " " " ultra-violet light.
4. " " " sunlight.
5. " " " metals in contact with the gas.
6. " " " the action of ultra-violet light on a negatively electrified zinc plate.
7. " " " the discharge of electricity from a pointed platinum wire.

In addition the behaviour of the nuclei in an electric field has been studied, with the object of distinguishing between "ions" and nuclei which carry no charge of electricity.

1. The action of strong X-rays on the gas differs from that of weak rays† merely in the number of nuclei produced, the supersaturation required to cause water to condense on the nuclei remaining unaltered. The value of v_2/v_1 corresponding to this degree of supersaturation (approximately fourfold) is equal to 1·25.

2. Uranium compounds, whether inside the expansion apparatus in immediate contact with the gas or contained in a glass bulb outside the apparatus‡, produce nuclei requiring the same degree of supersaturation as those produced by X-rays.

* 'Phil. Trans.,' A, vol. 189 (1897), p. 265; 'Camb. Phil. Soc. Proc.,' vol. 9 (1897), p. 333.

† 'Phil. Trans.,' *loc. cit.*

‡ 'Camb. Phil. Soc. Proc.,' *loc. cit.*

(Expansion experiments probably furnish one of the most delicate methods of detecting these rays.)

3. Ultra-violet light acting on moist air or oxygen produces nuclei which, when the radiation is weak, require quite as great a degree of supersaturation to cause water to condense on them as those produced by X-rays. With stronger radiation, however, the nuclei appear to grow, the expansion required to produce a cloud now depending on the strength of the radiation and on the time for which the gas has been exposed to the rays before expansion. With very strong ultra-violet light the growth of the nuclei, even in unsaturated air, continues till they become visible (as stated in a preliminary note).* The phenomena are then like those observed by Tyndall with certain vapours exposed to ordinary light. That nuclei are produced when ultra-violet light enters an expansion apparatus through a quartz window was discovered by Lenard and Wolff, but they believed them to arise from disintegration of the quartz. That these nuclei arise not at the quartz, but throughout the volume of the air exposed to the rays, is capable of experimental proof in a variety of ways. In hydrogen even strong ultra-violet light produces comparatively few nuclei, these requiring also as great a degree of supersaturation as the nuclei produced by X-rays in order that water may condense on them.

4. Sunlight produces in air nuclei which require large expansions (v_2/v_1 about 1·25) in order that water may condense on them.

5. Certain metals in moist air produce nuclei, always requiring great supersaturation in order that condensation may take place on them. The supersaturation required is generally as great or greater than that required in the case of X-ray nuclei. In the presence of amalgamated zinc dense fogs are obtained with expansions, which in the absence of the metal only result in the formation of a very few drops. Clean surfaces of zinc or lead have a similar but much slighter effect; with copper or tin it is inappreciable. These phenomena are obviously closely connected with the effects which these metals exert on a photographic plate, studied by Russell and others.

6. Ultra-violet light acting on a negatively electrified zinc plate produces condensation nuclei, as was proved by the steam jet experiments of Lenard and Wolff. The nuclei, however, are not, as these observers supposed, produced by disintegration of the metal, for expansion experiments show that they are identical with the nuclei produced by X-rays with respect to the degree of supersaturation required to cause condensation to take place on them, and therefore entirely unlike dust particles. In hydrogen the maximum number of drops in the fogs which result on expansion is obtained with comparatively weak fields; no nuclei are produced when the zinc is positively electrified.

7. The discharge from a pointed platinum wire in moist air or hydro-

* 'Camb. Phil. Soc. Proc.,' vol. 9, p. 392, 1898.

gen produces nuclei which also require approximately the same expansion, $v_2/v_1 = 1 \cdot 25$, in order that condensation may take place on them. If the expansion be made while the discharge is taking place no fog is obtained with smaller expansions. The results are not so simple if the expansion be made after the discharge has ceased, apparently on account of some secondary effect of the discharge causing the nuclei to grow. No nuclei were produced unless a glow could be observed at the point of the wire.

Effect of an Electric Field.—When air exposed to X-rays is enclosed by two parallel plates, between which a sufficient difference of potential is maintained, the fogs obtained on expansion are very much less dense than in the absence of the electric field, and if the rays be turned off before expansion all the nuclei are found to have been removed, whereas without any electric field a fog is obtained even if the expansion be not made till some seconds after the rays have been cut off. This behaviour of the nuclei proves them to be charged particles or "ions." The nuclei produced by uranium rays behave in a similar manner; those produced by the action of ultra-violet light on moist air, or by the presence of metals, are entirely unaffected by an electric field. They are therefore not ions but uncharged nuclei. (Since the nuclei produced by the action of metals on air or by weak ultra-violet light require just as great a degree of supersaturation as those produced by X-rays to cause water to condense on them, the difference in the behaviour of the two classes of nuclei can scarcely be due to a difference in size.) The nuclei which escape from a negatively electrified plate under the influence of ultra-violet light are of course charged.

It follows from the experimental results described in this paper that the passage of electricity through gases is effected by charged particles which have an identical effect as condensation nuclei, whether the conduction is the result of exposure of the gas to X-rays or uranium rays, or of the action of ultra-violet light on a negatively charged zinc plate, or consists in the escape of electricity from a pointed platinum wire. In all cases the degree of supersaturation required to make condensation take place on these particles is approximately fourfold.*

The nuclei which are produced and grow (in air or oxygen) under the action of ultra-violet light are uncharged, in their initial stages at least; they are therefore not electrified water drops. It is possible that they are water drops containing in solution some substance, perhaps H_2O_2, produced within them by the action of the ultra-violet light in quantities sufficient to counterbalance the effect of the curvature of the surface upon the vapour pressure necessary for equilibrium.

* 'Phil. Trans.,' *loc. cit.*

"The Origin of the Gases evolved on heating Mineral Substances, Meteorites, &c." By MORRIS W. TRAVERS, D.Sc. Communicated by Professor W. RAMSAY, F.R.S. Received November 14,—Read November 24, 1898.

The study of the gases evolved on heating mineral substances has been made the subject of a number of investigations, and the results have formed a basis for speculations as to the origin and history of these substances.

In a paper entitled "The Gases enclosed in Crystalline Rocks and Minerals,"* Professor Tilden has suggested a theory to account for the evolution of gases by mineral substances under the influence of heat. He considers that the gases, which are given off on heating certain rocks and minerals, are actually present in those substances in the gaseous state, enclosed in small cavities under high pressure. To account for their presence in these cavities he makes the suggestion, "that the rock crystallised in an atmosphere rich in carbon dioxide and steam, which had been, or were at the time, in contact with some easily oxidisable substance, at a moderately high temperature. Of the substances capable of so acting, carbon, a metal, or the protoxide of a metal, present themselves as most probable."

Beyond the fact that minerals give off gases when heated, there is, except in the case of carbon dioxide, no direct experimental evidence to show that these substances really contain the gases in the free state. In many cases no cavities can be seen in thin sections of the mineral, and as a mineral yields about the same quantity of gas when ground to a fine powder as when it is only broken into small pieces, the cavities must be assumed to be very minute if they exist at all. I do not contend that it is impossible for a rock to contain considerable quantities of gases enclosed in cavities, but I propose to prove experimentally, that at least in some cases where a mineral or rock yields gases other than carbon dioxide on heating, those gases are produced during ignition, by the interaction of its non-gaseous constituents. I shall first consider the formation of carbon monoxide and hydrogen from minerals and rocks which contain as active constituents only water, carbon dioxide, and ferrous oxide.

A glance through the results of the analyses of some such substances, and of the gases evolved by heating them, will show that the quantity of hydrogen and carbon monoxide produced bears a certain relation to the quantity of ferrous oxide and to the quantity of water given off on heating the substance. In the following table the quantities of ferrous oxide and water are expressed in terms of weight *per cent.*; the gases in cubic centimetres per gram of mineral.

* 'Roy. Soc. Proc.,' vol. 60, p. 453.

Mineral.	Locality.	FeO.	H_2O.	H_2.	CO.	CO_2.
Chlorite.........	Zoptan, Moravia	10·6	4·6	2·180	0·494	0·123
Serpentine[1] ...	Zermatt	2·7	9·5	0·800	none	none
Gabbro	Isle of Skye	6·1	1·5	0·490	none	none
Mica	Westchester, Pennsylvania	1·4	0·13	0·08		0·150
Foliated talc[2]...	Greiner, Tyrol...	0·4	4·5	0·04		0·070
Felspar[3]	Peterhead.........	2·1	1·00	0·214		1·201

[1] Analysis by Miss E. Aston, 'Quart. Jour. Geol. Soc.,' 1896, vol. 52, p. 456.

[2] The talc lost only 0·06 per cent. of water when heated in the hard glass tube, remainder came off at a very bright red heat.

[3] The felspar contained free iron.

The estimation of the ferrous iron was conducted in the following manner. The sulphuric acid (30 per cent.), previously boiled and cooled in a corked flask, was poured into the bottom of a thick-walled glass tube. The mineral was weighed out into a small test-tube with a glass rod sealed to the bottom of it to support it above the sulphuric acid in the large tube during exhaustion. The tube was drawn out at the end as in fig. 1, attached to a Töpler pump by a rubber tube, exhausted, and sealed at the capillary. After heating to 170°—till the mineral was entirely decomposed—the tube was again attached to the pump as in fig. 2, the point of the capillary was broken inside the rubber tube and the gas contained in it was pumped out and analysed. The tube was afterwards cut open and the ferrous sulphate was estimated by titration with a solution of potassium permanganate. In the case of the mica it was necessary to use strong sulphuric acid to decompose the mineral.

In general, the gas contained in the sealed tube consisted only of carbon dioxide. In the case of the felspar from Peterhead granite, and of certain helium-yielding minerals, the gas also contained hydrogen. I shall deal with these minerals separately.

That the minerals, which on heating give hydrogen and carbon monoxide, give neither of these gases when decomposed by means of dilute sulphuric acid, is almost sufficient evidence to show that the gases are not present in the minerals in a free state, *either occluded*, or enclosed in cavities. That the amount of hydrogen and carbon monoxide produced by heating a mineral is, as I have shown, proportional to the amount of ferrous oxide, water, and carbon dioxide present in it, may be taken as evidence that the gases are produced by the interaction of these substances when the mineral is heated, according to the equations:—

$$2FeO + H_2O = Fe_2O_3 + H_2$$
$$2FeO + CO_2 = Fe_2O_3 + CO$$

If this is the case, the mineral, after ignition *in vacuo*, should contain less ferrous oxide than it did originally; the difference between the quantities of ferrous oxide before and after heating should be equivalent to the hydrogen and carbon monoxide evolved; and these may be calculated from the equations written above.

One c.c. of hydrogen or carbon monoxide is equivalent to 0·006428 gram of ferous oxide.

In order to prove this experimentally, I selected a chlorite from Zoptan in Moravia. This mineral contained 10·60 per cent. of ferrous oxide, 4·79 per cent. of water, and consequently gave a considerable quantity of hydrogen on heating. It was also quite free from sulphides, which seriously interfere with the accurate estimation of ferrous oxide.

A weighed quantity, about 10 grams, of the chlorite was heated in a hard glass tube till it ceased to give any gas. The gas was collected and analysed, and the loss of weight of the mineral was determined, by weighing the hard glass tube before and after heating.

Analysis of gas expressed in cubic centimetres per gram—

CO_2	0·123
CO	0·094
H_2	2·180

Loss of weight on heating, 4·79 per cent.

The mineral was allowed to cool *in vacuo* to prevent the oxidation of the ferrous oxide. The ferrous oxide was estimated in a sample of the mineral before and after heating.

Ferrous oxide in mineral before heating—

(*a*) 10·62 per cent.; (*b*) 10·60 per cent.; mean, 10·61 per cent.

Ferrous oxide in mineral after heating—

(*a*) 9·70 per cent.; (*b*) 9·61 per cent.; mean, 9·65 per cent.

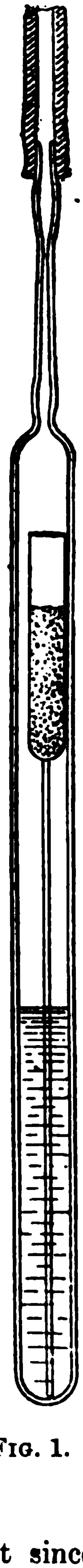


Fig. 1.

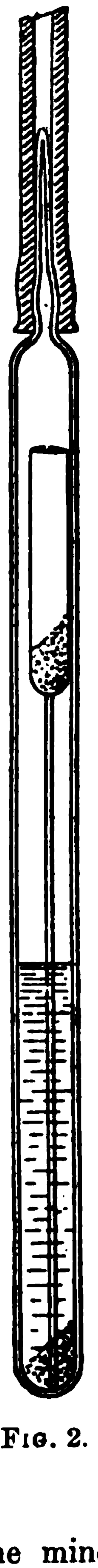


Fig. 2.

But since the mineral lost weight to the extent of 4·79 per cent. on heating, a correction must be applied.

Ferrous oxide in mineral after heating, corrected for loss of weight of mineral	9·18 per cent.
Difference in amount of ferrous oxide present in mineral before and after heating	1·43 ,,

The amount of ferrous oxide oxidised to ferric oxide can also be calculated from the quantities of hydrogen and carbon monoxide collected.

2·180 c.c. per gram of hydrogen is equivalent to	1·401 per cent. of FeO
0·094 c.c. per gram of carbon monoxide is equivalent to	0·057 ,, ,,
Total...............	1·458 ,, ,,

This number agrees very closely with that already obtained.

The application of this method of investigation is very limited, as it is difficult to obtain minerals which are quite free from sulphides and carbonaceous matter, and which at the same time give on heating a sufficient quantity of carbon monoxide and hydrogen. Other specimens of chlorite gave large quantities of hydrogen on heating, but they were found to contain sulphides.

The minerals and rocks, which had been investigated up to this point, had all been of igneous origin. At Professor Bonney's suggestion, I next proceeded to determine whether minerals of aqueous origin, containing ferrous oxide, water, and carbonates, would give hydrogen and carbon monoxide on heating. Professor Bonney kindly obtained for me a specimen of chalk marl, rich in glauconite, from the Cambridge beds.

The glauconite was freed from chalk as far as possible by washing with dilute hydrochloric acid. The residue, seen under the microscope, appeared to consist of foraminiferous casts, with adhering grains of glauconite. The sample taken for the experiment contained 1·33 per cent. of ferrous oxide, 15 per cent. of water, and 13·7 per cent. of calcium phosphate.

Heated in a hard glass tube to a dull red heat for some time, only 5·7 per cent. of the water was given off with a considerable quantity of gas, which was found on analysis to have the following composition:—

CO_2	4·518 c.c. per gram.
H_2	3·128 ,,
CO	0·083 ,,
CH_4, &c.	0·204 ,,

On heating to a bright red heat in the flame of a Bunsen burner, the remaining water and more gas was given off:—

CO_2	13·38 c.c. per gram.
H_2, &c.	0·25 ,,

The greater part of the hydrogen and hydrocarbons is probably produced by the breaking down of organic matter in the mineral. This organic matter may be due to infiltration from the surface, but, considering the treatment the mineral had received, it is more probably contained in the foraminiferous casts, the decomposition products of their original occupants.

The presence of manganous oxide in minerals appears also to favour the production of hydrogen and carbon dioxide; indeed, it has long been known, that when moist manganous carbonate is heated, these gases are always produced along with carbon dioxide.

The first reactions which take place when the mineral is heated, may be expressed by the equations :—

$$MnO + H_2O = MnO_2 + H_2$$
$$MnO + CO_2 = MnO_2 + CO.$$

But it was found that when cerite, a mineral which contains a considerable quantity of manganous carbonate was heated, the gas contained not only hydrogen, but oxygen, the product of a reaction taking place at a somewhat higher temperature :—

$$3MnO_2 = Mn_3O_4 + O_2.$$

Analysis of the gas obtained by heating finely powdered cerite gave the following results :—

CO_2............	15·0 (about) c.c. per gram.
CO	1·554 ,,
H_2	3·905 ,,
O_2	0·406 ,,

A cobalt ore containing only the higher oxides of manganese gave about 10 c.c. of oxygen per gram of minerals.*

Yttrotantalite, samarskite, &c., containing lower oxides of uranium, also give small quantities of hydrogen when heated. The hydrogen may be produced according to the equation :—

$$3UO_2 + 2H_2O = U_3O_8 + 2H_2.$$

The quantity of hydrogen is not, however, very large.

Minerals containing sulphides usually give a mixture of hydrogen, sulphuretted hydrogen, and sulphur vapour when heated. The quantity of gas is often very large, but the reaction is too complicated for direct investigation. It is probable that the sulphides react with water

* 'Roy. Soc. Proc.,' vol. 60, p. 444.

forming sulphuretted hydrogen, which at the temperature of the reaction is decomposed into sulphur and hydrogen.

It is known that certain crystalline minerals contain liquid hydro carbons enclosed in cavities. It is also possible that the methane, and other hydrocarbons sometimes present in the gases obtained by heating mineral substances, may be produced by the destructive distillation of bituminous matter infiltrated into it, or from vegetable matter, particularly if the specimen has been long exposed on the surface.

Nitrogen I have only rarely obtained by heating minerals. Malacone* yields it in larger quantity, compared with the total quantity of gas evolved, than any other mineral which I have examined, but even in that particular case only a very small quantity of the gas is obtained. It is possible that a finely powdered mineral may on standing condense air on its surface. Dr. G. McGowan tells me that when a sample of china clay was heated in a current of hydrogen a small quantity of ammonia was given off. Certain bituminous shales, in which the organic matter is certainly of animal origin, are known to contain large quantities of ammonium salts; the gases evolved under the influence of heat would certainly contain nitrogen. Nitrogen and its oxides might also be the product of the interaction at a high temperature of nitrates infiltrated into the rock, and silica. That Davy obtained nitrogen from quartz is highly improbable. It is more likely that the gas was introduced accidentally in the course of the experiment.

I stated earlier in this paper that the felspar from Peterhead granite exhibited certain peculiarities, and that I should describe it separately. The granite consisted of very large crystals of quartz and felspar with a comparatively small quantity of mica of a dark colour. On heating, a mixture of carbon dioxide, carbon monoxide, and hydrogen was given off.

In order to ascertain whether these gases were derived from each of the minerals, or whether any particular one gave more gas on heating than the others, I crushed a large quantity of the granite in an iron mortar and picked out the constituents, separating them from one another as far as possible. The mica was easily obtained quite pure, but the quartz and felspar could not without very great trouble be separated from one another; in fact, the felspar always contained a considerable quantity of the latter mineral.

By heating weighed quantities of the component minerals to red heat in hard glass tubes, and collecting and analysing the gases, the following results were obtained. The results are expressed in cubic centimetres of gas per gram of mineral :—

	Quartz.	Mica.	Felspar.
Carbon dioxide	0·225	none	0·172
Carbon monoxide	0·074	none	0·059
Hydrogen		0·164	0·218

* 'Roy. Soc. Proc.,' 1897, vol. 60, p. 444.

These results show that the mica and felspar are responsible for nearly the whole of the hydrogen, while the greater part of the carbon dioxide is derived from the quartz. The carbon dioxide is probably present in cavities in the quartz, but if the hydrogen were also present in cavities, one would expect that it would be also obtained in greatest quantity from the quartz, the only mineral of the three which has no cleavage; one would not certainly expect to obtain it from the mica.

As the felspar gave off gas in the cold when treated with dilute sulphuric acid, a weighed quantity of the mineral was placed in a thick-walled tube, with a second tube containing 50 per cent. sulphuric acid. The tube was drawn to a point, exhausted, and sealed in the manner already described. After heating for twenty-four hours to 170° the gas was pumped out and analysed, and the ferrous sulphate in the solution was determined.

The following results were obtained from two samples of felspar from different parts of the same block of granite. The quantities of gas are expressed in cubic centimetres per gram :—

	(*a*)	(*b*)
Ferrous oxide in solution...............	4·02 p.c.	2·05 p.c.
Hydrogen with trace of hydrocarbon	3·50 „	1·99 „
Carbon dioxide..........................	0·20 „	0·90 „

From these results I was led to suppose that the felspar contained both free iron and ferrous oxide, since if we calculate the amount of iron which would be equivalent to the hydrogen evolved, we find that expressed in terms of ferrous oxide,

3·50 c.c. of hydrogen per gram = 2·25 per cent. of ferrous oxide.
1·99 „ „ = 1·23 „ „

Quantities which are considerably less than the quantity of ferrous oxide found by titration. On the other hand, it is possible that some of the hydrogen is taken up by the ferric compounds present, lessening the yield of hydrogen, and increasing the quantity of ferrous sulphate in the solution.

There is, unfortunately, no accurate method of estimating the free iron in minerals. The copper method apparently fails in the presence of hydrated ferric oxide, on account of the secondary reactions which take place between that compound and copper sulphate, and subsequently between the ferric sulphate and precipitated copper.

In order to prove that a considerable amount of free iron was present in the felspar, two weighed quantities of felspar from the same sample were heated with 50 per cent. sulphuric acid and 96 per cent. acid respectively. The gases were subsequently pumped out of the tubes and analysed, and the ferrous sulphate was estimated in the liquid.

The following results were obtained :—

(*a*) With strong sulphuric acid, carbon dioxide and sulphur dioxide	1·26	c.c. per gram.
Hydrogen and trace of hydrocarbons............	0·49	„
Total	1·75	„
Ferrous oxide .... 2·07 per cent.		
(*b*) With 50 per cent. sulphuric acid, carbon dioxide	0·23	„
Hydrogen and trace of hydrocarbons............	1·64	
Total	1·87	„
Ferrous oxide .... 2·26 per cent.		

These figures indicate that the sulphur dioxide and hydrogen produced by the action of the strong acid is just equivalent to the hydrogen produced by the dilute acid, indicating that the felspar contains metallic iron, which is probably the source of the whole of the hydrogen evolved when the mineral is treated with acid. The hydrogen and carbon monoxide given off on heating the felspar are produced by the action of water and carbon dioxide on ferrous oxide and metallic iron present in the mineral.

Both the gas obtained by heating the felspar and gases obtained from the sealed tube experiments contained traces of hydrocarbon. This may be accounted for on the assumption that the free iron in the felspar contains a small quantity of carbide.

Meteorites.

It has long been known that considerable quantities of gas are evolved when meteorites are heated *in vacuo*. The gas usually consists of hydrogen, carbon monoxide, carbon dioxide, and hydrocarbons, in varying proportions, and attempts have been made to draw conclusion as to the origin of different meteorites from the results of analysis of the gases obtained by heating them. These speculations are based upon the supposition that the gases evolved on heating are present as such in the meteorite, occluded from the atmosphere in which it previously existed.

Beyond the gases already mentioned, the results of many observers appear to show that nitrogen is also invariably present. Among those that I have examined the gas evolved on heating contained, in no single instance, a trace of nitrogen.* Graham† found as much as 10 per cent. of nitrogen in the gas from the Lenarto meteorite, and

* 'Roy. Soc. Proc.,' 1897, vol. 60, p. 442.

† 'Roy. Soc. Proc.,' 1867, vol. 15, p. 502.

Mallett* found 10 per cent. of nitrogen in the gas from a meteorite from Augusta Co., Virginia. Wright† examined a large number of meteorites, and found that in almost every case the gas contained nitrogen. In this case, however, the presence of nitrogen can easily be accounted for, as no particular precautions were taken with regard to the thorough exhaustion of the apparatus employed. A Sprengel pump was used, "which was kept running till the air was thoroughly removed, *as could be seen by the gauge.*" Dewar‡ found nitrogen, in quantities not exceeding 4 per cent. of the total gas, in the gases from samples of meteorites and graphite.

In one single case a meteorite has been found to yield a trace of helium on heating.§

With regard to the carbon dioxide and combustible gases, it is difficult to obtain direct evidence as to their origin. In the case of meteorites containing bituminous matter and carbonaceous nodules, the evolution of these gases may be attributed to the destructive distillation of their constituents. Meteorites of the stony variety appear to evolve more carbon dioxide and hydrocarbons, and less hydrogen and carbon monoxide, than those which are of a metallic nature. Several specimens of stony meteorites have been carefully examined by Wright and Dewar with the following results:—

Wright (*loc. cit.*) found that in the case of a stony meteorite from Iowa Co., Iowa, the carbon dioxide was given off at a very low temperature. The following table shows the composition of the gas given off at different temperatures:—

	At 100°.	At 250°.	Below red heat.	Dull red heat.	Full red heat.
CO_2	95·46	92·32	42·27	35·82	5·56
CO	0·00	1·82	5·11	0·49	0·00
H_2	4·54	5·86	48·06	58·51	87·53
N_2	0·00	0·00	4·56	5·18	6·91

The meteorite lost about 10 per cent. of its weight of water on heating. The water was allowed to collect in the apparatus, and as no drying reagent was used, it is easy to account for the presence of hydrogen in the gas. The carbon dioxide may have been present as an unstable hydrated carbonate, or in the state of occlusion in the pores of the substance.

Dewar (*loc. cit.*) showed that a meteorite of a similar nature was capable of reabsorbing water and carbon dioxide after the gases had been removed by heating *in vacuo*. The following results were obtained:—

* 'Roy. Soc. Proc.,' 1872, vol. 20, p. 365.

† 'Amer. J. Sci.,' [3], vol. 9, pp. 294 and 459; vol. 10, p. 44; vol. 11, p. 254.

‡ 'Roy. Inst. Proc.,' 1886, p. 545.

§ *'Nature,' 1896.*

	Gas in volumes of meteorite.	CO_2.	CO.	H_2.	N_2.
After 24 hours	0·61	54·0	—	42·4	3·6
After 6 days more ...	2·47	47·0	5·0	47·0	1·0
After 8 days more ...	0·63	96·1	2·0	1·5	—

The quantity of water reabsorbed after the second heating was very small, and it is interesting to note that the quantity of hydrogen evolved during the subsequent heating was also very small. From this it would appear that the hydrogen was produced directly from the water. There is no evidence to show whether the carbon dioxide entered into combination with some constituent of the meteorite, or not.

Meteorites of the second class usually consist chiefly of metallic iron, nickel, &c., with small quantities of crystalline minerals, such as olivine. The presence of these minerals, which are usually hydrated silicates containing ferrous oxide, might in themselves account for the formation of hydrogen. The carbon monoxide might be produced by the interaction of carbon dioxide, the product of decomposition of a carbonate, with the metallic iron. The small quantities of hydrocarbon, which are also present in the gas, and which appear to belong to the saturated series, might be produced by the action of water, which is invariably present, upon metallic carbides. The changes which take place are probably of a complicated nature.

In order to ascertain whether a sample of meteoric iron actually contained occluded or enclosed gases, the following experiment was performed. A piece of meteoric iron was cut into fine shavings, which were carefully cleaned. The metal was divided into two portions; one part was heated in a sealed tube with copper sulphate and water, in the manner already described, the other was heated *in vacuo*. The gases evolved were in each case collected and analysed.

	By action of heat.	Copper sulphate experiment.
Hydrogen	0·322 c.c. per gram	0·014 (Hydrogen, carbon monoxide and hydrocarbons together)
Carbon monoxide and hydrocarbons...	0·164 ,, ,,	
Carbon dioxide	2·222	0·739

The trace of hydrogen which was produced during the copper sulphate experiment may well be attributed to secondary relations between the metal and the salts in solution.

It would appear then that the gases produced by the action of heat upon meteorites are not present as such, but are the products of decomposition of their non-gaseous constituents. It is therefore impossible to draw conclusions as to the former history of a meteorite from the nature of the gases which it gives on heating.

Helium and Argon.

With regard to the state in which helium is present in the minerals from which it is obtained by the action of heat, there is at present no conclusive evidence. It must be present under one of three conditions:—

I. In combination with some constituent of the mineral.
II. Occluded; or in solution in the mineral.
III. Enclosed in cavities under pressure.

Microscopic examination of the minerals, from which helium has been derived, has failed to reveal the presence of cavities, and, indeed, if we assume that cavities exist, and that they contain all the helium present in the mineral, we have to make further assumptions to explain why the helium escapes when the mineral is heated, although in most cases no disintegration takes place. It seems improbable that the helium is present in the state of solid solution or occlusion, a condition of which we yet know little, for unless we assume that the rate of diffusion of the helium through the mineral in which it is dissolved is infinitely small, the gas should long ago have escaped into the atmosphere. Further, it is probable that the supposed cases of solution of gases, like hydrogen, in solids such as platinum, palladium, &c., are really cases of chemical combination, as has recently been proved by the researches of Ramsay, Mond, and Shields, and even in meteoric iron it is improbable that the hydrogen is not present as such, but is the product of secondary reactions. Indeed, in the state of our knowledge at present it is impossible to draw a distinction between occlusion and chemical combination.

We are, therefore, forced to the conclusion that helium is present in the minerals in the state of combination with one of its constituents. It may be well to review such positive evidence as exists in favour of such a supposition.

In the first place the gas is not found generally dispersed among crystalline mineral substances, but seems only associated with certain elements, uranium, yttrium, &c., in minerals which are invariably vein products.

It has been pointed out by Professor Ramsay and the author,* that in certain cases the evolution of helium from the mineral is accompanied by a considerable evolution of heat, and in one case by a considerable decrease in density. This matter has been dealt with fully in the paper (*loc. cit.*), and is considered as evidence in favour of chemical combination. Julius Thomsen† has confirmed this observation.

* 'Roy. Soc. Proc.,' vol. 62, p. 325.
† 'Zeit. Phys. Chem.,' vol. 25, p. 112.

A series of experiments were undertaken to determine the conditions under which the minerals gave off helium. It was found that from clèveite helium was evolved, but very slowly, at the temperature of boiling quinoline, and somewhat faster at the temperature of boiling sulphur. At a bright red heat a considerable quantity of helium was obtained, but in no case did the mineral lose the whole of its helium under the influence of heat alone.

By heating clèveite to redness for some hours a mixture of helium, hydrogen, carbon dioxide, and carbon monoxide was obtained; the gas came off readily at first, and appeared finally to cease altogether.

A quantity of the same sample was completely decomposed by means of sulphuric acid (30 per cent.) in an exhausted sealed tube. The results are given below:—

	By heating the mineral.	By action of sulphuric acid.
He	1·487 c.c. per gram.	3·201 per cent. per gram.
H_2	0·367 ,,	0·333 ,,
CO_2	2·298 ,,	

It will be noticed that only half the helium is given off on heating the mineral; this makes it appear probable that if the helium is present in the mineral originally in a state of binary combination the decomposition takes place according to the equuation:—

$$XHe_2 = XHe + He.$$

A specimen of fergusonite was also examined, but the results were not altogether satisfactory, as it was found to be practically impossible to completely decompose the mineral by the action of 30 per cent. sulphuric acid; strong sulphuric acid appeared to be still more inactive. When fused with acid potassium sulphate a larger yield of helium was obtained but the mineral was not completely decomposed. The following figures indicate that about half the helium contained in the mineral is liberated under the influence of heat.

	By action of heat on mineral.	By 30 per cent. sulphuric acid	Fusion with acid sulphate.
He ...	1·041 c.c. per gram.	1·434 c.c. per gram.	1·813 c.c. per gram.
H_2 ...	0·231 ,,	0·163 ,,	
CO, &c.	0·326 ,,		

It is somewhat significant that both clèveite and fergusonite yield hydrogen when decomposed by sulphuric acid. If the helium were present in combination with a metal it would eventually be liberated as a hydride. It is probable that the hydride would be a very unstable compound and would decompose at the moment of formation. Part of the hydrogen would probably be taken up by the ferric and uranic compounds, and part would escape in the gaseous state. A similar

reaction would take place between sulphuric acid, and an iodide, in the presence of a reducible substance, at a higher temperature.

Conclusions.

It would appear that the only evidence on which the assumption that gases of a permanent character, such as hydrogen, carbon monoxide, nitrogen, helium, and argon, exist in the free state in the mineral substances from which they are evolved on heating, rests on certain observations with regard to the cavities which can sometimes be detected by microscopic examination.

The cavities may be either apparently empty or they may contain liquid, and when the mineral is warmed the liquid disappears at a temperature which is a few degrees below the critical point of carbon dioxide or of some hydrocarbon. The fact that the critical temperature of the liquid is a little below the point corresponding to carbon dioxide, in the case of a mineral containing that substance, is not, however, of very great significance as pointing to the presence of a permanent gas. A small quantity of methane would produce the same result.*

Further, although it can be shown that *compact* minerals do enclose carbon dioxide and hydrocarbons, gases which can easily be liquefied, the analogy cannot be extended to gases such as hydrogen and helium in connection with minerals like chlorite, mica, and clèveite, which exhibit many cleavages.

On the other hand, there is, as I have endeavoured to show, a considerable amount of evidence in favour of the theory which I have put forward:—That in the *majority of cases* where a mineral substance evolves gas under the influence of heat, the gas is the product of the decomposition or interaction of its *non-gaseous* constituents at the moment of the experiment. The results of such experiments cannot, therefore, serve as basis for speculation as to origin and history of the substances in question.

"The Electrical Conductivity and Luminosity of Flames containing Vaporised Salts," By ARTHUR SMITHELLS, H. M. DAWSON, and H. A. WILSON. Communicated by Sir H. E. ROSCOE, F.R.S. Received October 24,—Read November 17, 1898.

(Abstract.)

1. *Object of the Investigation.*

No general consensus of opinion appears to exist as to the mode by which the metal of an alkali salt is liberated when the salt is vaporised

* Kuenen, 'Phil. Mag.,' 1897.

in a flame. By some the liberation is supposed to be effected thermally by chemical dissociation; others suppose that the salt is converted into hydrate or oxide, and then reduced by the flame gases. It is also noteworthy that, however the metal be liberated, and however oxidisable it may be, light is emitted from parts of the flame where oxidising gases are in abundance, and where even less oxidisable metals in the massive state are rapidly oxidised.

The primary object of the experiments described in this paper is to ascertain whether the luminosity imparted by vaporised salts to flames is related in a definite manner to the electrical conductivity of the salt vapours. The conductivity of vaporised salts in flames has been investigated by Arrhenius,* who concluded that it was of an electrolytic character. In the case of alkali salts, Arrhenius supposes that the salt vapour is acted upon by the large quantity of water vapour in the flame, in accordance with the following equation—

$$M'X' + H_2O = M'OH + HX',$$

and that the metallic hydrate so formed undergoes partial electrolytic dissociation into the ions $\overset{+}{M}$ and $(\overset{-}{HO})$. From the analogy stated to exist between dilute solutions of solids and matter in the gaseous state, and from his own theory that in dilute solutions electrolytes are in greater or less degree dissociated into their ions, Arrhenius was led to believe that electrolytes distributed in small concentration throughout a gas, would likewise be electrolytically dissociated,—a view to which his results as above stated, are conformable.

Since, according to the electrolytic dissociation theory of Arrhenius as applied to dilute solutions, the metallic ion in virtue of its electric charge can persist in an oxidising medium, it appeared that if the same theory were really applicable to salts vaporised in flames, it would afford an explanation both of the liberation of the element, and of its persistence in the midst of an oxidising atmosphere of flame gases.

Another consideration appeared to favour this hypothesis. According to Arrhenius, the conductivity of a salt vapour is proportional to the square root of its concentration in the flame, and according to Gouy† the luminosity of a flame coloured by an alkali salt also follows within certain limits, the same law.

The motive of the present authors was to test the above hypothesis, and incidentally to gain increased knowledge of the circumstances that govern the electrical conductivity of vaporised salts.

* 'Wied. Ann.,' vol. 42, p. 18, 1891.

† Gouy, 'Ann. Chim. Phys.,' vol. 18, p. 5, 1879.

2. *Apparatus.*

The apparatus employed consisted essentially of the arrangement used in other investigations of flame,* whereby the two cones that constitute the flame of a Bunsen burner, can be separated widely and maintained apart for any length of time. The gas and air supplies were regulated with great care, and the air supply was made to actuate a sprayer, whereby an extremely fine spray of any salt solution could be led into the flame. The devices used in regulating the gas and air supplies, and the precautions necessary in the construction and use of the sprayer, are described in the paper.

The electrode system which usually consisted of two coaxial cylinders of platinum-iridium alloy, was fixed symmetrically in the space between the two cones of the flame.

The source of electricity consisted of three accumulators, from which by means of a German-silver wire, 20 metres long, and two contact pieces, any E.M.F. up to 5·7 volts could be used. For higher E.M.F.'s Leclanché cells were used.

3. *Method of Working.*

After the apparatus had been adjusted, the current in either direction between the electrodes was measured by a Kelvin high resistance galvanometer for a series of E.M.F.'s. The constancy of the apparatus was tested at intervals during the progress of the experiments by measuring the conductivity due to a $\frac{1}{10}$th normal solution of potassium bromide, and the results were satisfactory.

4. *Conductivity of the Free Flame.*

From the observed conductivity due to a salt, it was necessary to deduct the conductivity of the flame gases alone, and that of the water in the spray. This value, which is very small, was determined by measurements made when distilled water only was sprayed.

5. *Unipolar Conduction.*

Considerable unipolar effects were noticeable in the experiments, and measurements of these are given in the paper.

6. *Measurement of the Concentration of Salt-vapour in the Flame.*

It was not necessary for the purpose of the enquiry to determine the absolute amount of salt between the electrodes; but for the purpose of instituting some comparison with the results of Arrhenius, a rough

* Smithells, 'Phil. Mag.,' vol. 39, p. 122, 1895.

measurement of the amount of salt was attempted by a photometric method. It appeared that the amount of salt conveyed to the flame was about eighteen times as much as in the corresponding experiments of Arrhenius, so that it was possible to investigate the conductivity of salt vapours at greater concentrations than was done by Arrhenius.

7. *Relation between Current Strength and Electromotive Force.*

Experiments were made with a large number of salts, and with a difference of potential between the electrodes varying from 0·01 volt to 45 volts. The results show that with small E.M.F.'s up to 0·2 volt, Ohm's law is accurately obeyed. With greater E.M.F.'s the law is not obeyed, the deviation becoming greater in increasing proportion as the E.M.F. is increased.

The general relationship between current strength and E.M.F. was expressed by Arrhenius as follows :—

$$C = Af(E),$$

where C is current strength, E the E.M.F., and A a constant dependent on the solution sprayed. This expression is only valid for the present results within certain limits. With the more concentrated solutions it is not applicable.

Acting upon a suggestion of Professor J. J. Thomson, the authors have found an equation capable of expressing the relationship between C and E in a remarkably complete way. This equation is based upon the work of Thomson and Rutherford* on the passage of electricity through gases exposed to Röntgen rays, a phenomenon which has several points of external resemblance to that of conduction through flames. The equation is—

$$C = i + k_1 E,$$

where i bears the same relation to the E.M.F. as the current in X ray conductivity. Tables are given in the paper showing to what extent the above equation expresses the results obtained.

8. *Influence of Temperature on Conductivity.*

Experiments were made in which the electrodes were raised or lowered, so as to bring them into regions of different temperature. The temperature differences were measured by means of a platinum platinum-rhodium thermocouple. The results showed that the conducting power of the salt vapour increased very rapidly with increasing temperature, and that at temperatures not greatly below those which the vapour attains in flames, the conductivity would become inappreciable.

* 'Phil. Mag.,' vol. 42, p. 392, 1896.

9. *Relation of Conductivity to Concentration of Solution sprayed and to the Nature of the Salt.*

Results are given for solutions of the following substances :—

a. Potassium salts :—Chloride, bromide, iodide, chlorate, nitrate, sulphate, carbonate, hydrate.
b. Sodium salts :—Fluoride, chloride, bromide, iodide, nitrate, sulphate, carbonate, hydrate.
c. Lithium salts :—Chloride, nitrate.
d. Rubidium salts :—Chloride, nitrate.
e. Cæsium salts :—Chloride, nitrate.
f. Hydrogen salts :—Chloride, sulphate.

The concentration of the solution varied from $\frac{1}{500}$th to $\frac{1}{2}$ normal. As an example of the range of work, it may be stated that potassium iodide was investigated with $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{100}$, $\frac{1}{500}$ normal solutions, in each case measurements being made for E.M.F.'s of 5·6, 0·795, and 0·227 volts. The set of measurements was repeated more than once, as a rule, in order to avoid errors.

The results show that at small concentrations equivalent solutions of all salts of the same metal impart the same conducting power to the flame. At higher concentrations this equality no longer holds good; the oxy-salts show a greater conducting power than the haloid salts, the difference increasing with increasing E.M.F.

Numbers proportional to the molecular conductivity are calculated for the various salts, and it is shown (*a*) that in general the molecular conductivity of a salt increases with increasing dilution; (*b*) that the oxy-salts of all alkali metals behave differently from the haloid salts; and (*c*) that at all concentrations investigated, the conducting power of the oxy-salts of any one metal is the same. It also appears that with increasing concentration the molecular conductivity of the oxy-salts passes through a minimum value.

In the case of the haloid salts the equation $C = k\sqrt{q}$ (where C is the conducting power, q the concentration, and k a constant) holds good to a certain extent, but this is not at all the case with the oxy-salts.

The conductivity increases with increasing atomic weight of the metal, the increase being more rapid in the case of the oxy-salts than in that of the haloids.

10. *Conductivity of Flames containing Acids.*

The conductivity of acids in the flame is very small in comparison with that of alkali salts. Ammonium salts being decomposed in the flame, behave like their acid component. Sulphuric acid is doubtless *also decomposed* in the flame. The conductivity of hydrochloric acid

was measured with a view to subsequent experiments, and was found to be—with a half-normal solution—five or six times as great as that of the vastly more concentrated water vapour which existed in the flame.

11. *Experiments with Decolorised Flames containing Salt-Vapours.*

When chloroform vapour is passed into a flame containing a salt-vapour, the colour is suppressed, owing to the large amount of hydrochloric acid formed. The conductivity of flames in this condition was determined and compared as nearly as was possible with the conductivity of flames containing the same amount of vapour, but no chloroform. The salts of lithium, potassium, and cæsium were used. It was found that the conductivity of the flames was not largely affected by the decolorisation. With small E.M.F.'s the conductivity was somewhat diminished, but with an E.M.F. of 5·6 volts an increase was always noticed.

12. *Conductivity of Salts vaporised in the Flame of Cyanogen.*

The view of Arrhenius is that salts are hydrolysed in flames by the water vapour present, and that the hydrate furnishes the ions. To gain some idea of the influence of water vapour, the cyanogen flame was chosen as a medium for the volatilisation of salts. Such a flame contains only the water coming from the sprayer. No differences were noticed in the behaviour of the several salts that would not have been found equally in a coal-gas flame.

The high general temperature reigning in a cyanogen flame causes a high degree of conductivity. A dry salt vaporised into a cyanogen flame from a platinum wire shows great conductivity, and thus it seems certain that the presence of water vapour in the flame is not necessary for the production of ions.

General Conclusions.

1. The authors conclude from their experiments, that the conductivity of vaporised salt is of an electrolytic character, but that there are features connected with it that distinguish it from electrolytic conduction in aqueous solution. Thus Ohm's law is only obeyed within certain limits, and the general relation between current strength and electromotive force can only be represented generally by a more complex expression.

2. The conductivities of different salts differ greatly, according to the electropositive constituent.

3. Among different salts of the same metal differences of conductivity

appear at the higher concentrations, but at low concentrations equivalent solutions have equal conductivity.

4. The conductivity of the haloid salts as a group is distinct from that of the oxy-salts.

5. The conductivity of the haloid salts of a metal among themselves increases with the increasing atomic weight of the halogen.

6. The conductivity of the oxy-salts of a metal is approximately equal, and approaches that of the hydrates.

7. The more easily oxidisable halogen salts are probably partly converted into oxide in the flame, so that their conductivity is composed of two parts.

8. The behaviour of the salts in flames supplied with chloroform vapour seems to establish the fact that the conductivity and the colour produced by the salt vapour are not due to a common cause.

The coloration of a flame by an alkali salt does not seem therefore to be connected with the ionisation of the salt. It must be attributed to the metal set free by a chemical process. This process consists probably in a reduction effected by the flame gases. An oxy-salt would, generally speaking, form in the first instance an oxide, which would then be reduced. In the case of haloid salts it seems also necessary to suppose that an oxide is intermediately formed, the metal then being liberated by reduction.

November 30, 1898.

Anniversary Meeting.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A full Report of the Anniversary Meeting, with the President's Address and Report of Council, will be found in the 'Year-book' for 1898–9.

The Account of the Appropriation of the Government Grant and of the Trust Funds will also be found in the 'Year-book.'

December 8, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Dr. Alexander Buchan was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had nominated as Vice-Presidents for the ensuing year—

The Treasurer.
Professor Bonney.
Mr. Story Maskelyne.
Dr. W. J. Russell.

The following Papers were read:—

I. "Effects of Prolonged Heating on the Magnetic Properties of Iron. (Second Paper.)" By S. R. ROGET, B.A. Communicated by Professor EWING, F.R.S.

II. "On the Topographical Anatomy of the Abdominal Viscera, especially the Gastro-Intestinal Canal in Man." By CHRISTOPHER ADDISON, M.D., B.S. (Lond.), F.R.C.S., Professor of Anatomy, University College, Sheffield. Communicated by Professor ALEXANDER MACALISTER, F.R.S.

III. "Mathematical Contributions to the Theory of Evolution. VI. Reproductive or Genetic Selection. Part I. Theoretical." By KARL PEARSON, "Part II. On the Inheritance of Fertility in Man." By KARL PEARSON and ALICE LEE. "Part III. On the Inheritance of Fecundity in Thoroughbred Race-horses." By KARL PEARSON, with the assistance of LESLIE BRAMLEY-MOORE.

IV. "'Nitragin' and the Nodules of Leguminous Plants." By MARIA DAWSON, B.Sc. (Lond. and Wales). Communicated by Professor H. MARSHALL WARD, F.R.S.

"Effects of Prolonged Heating on the Magnetic Properties of Iron. (Second Paper.)" By S. R. ROGET, B.A. Communicated by Professor EWING, F.R.S. Received October 26—Read December 8, 1898.

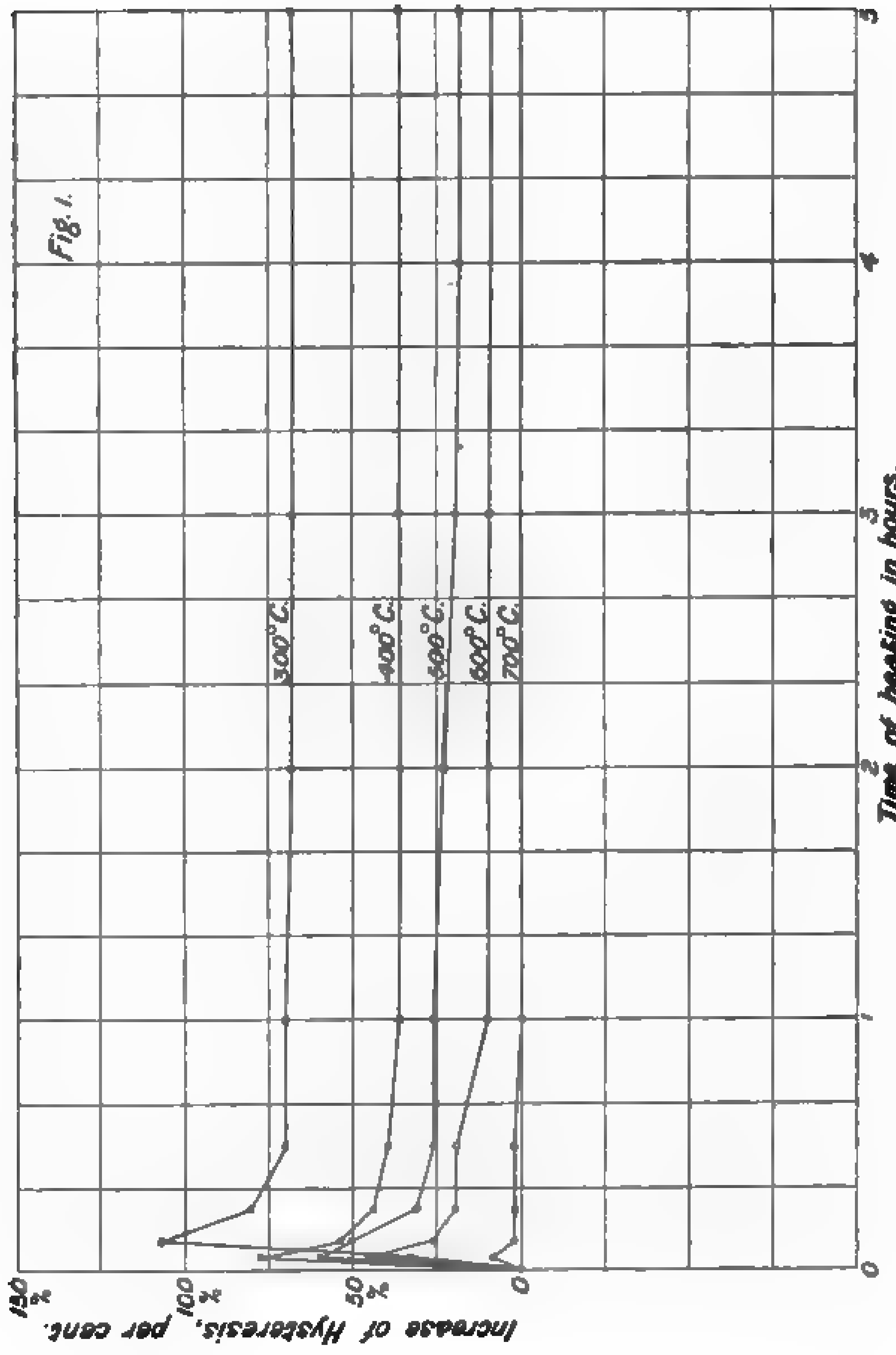
In a paper by the author, read before the Royal Society on May 12, 1898,* the results of some experiments were given showing the change in the value of the hysteresis of soft iron transformer plate when subjected to continued baking at temperatures not exceeding 200° C. The experiments there described have since been extended to higher temperatures, and the results for heating at temperatures up to 700° C. are given below.

The same specimens were used as in the former experiments, consisting of soft Swedish iron transformer plate which had been re-annealed. The baking at these high temperatures was carried on in a specially constructed electric oven heated by coils of platinum wire wound on a mica frame, inside a metal vessel completely surrounded by a lagging of silicate cotton except for a mica tube through which the specimens and thermometer were introduced. By this means, any desired temperature could be maintained up to a bright red heat, so that the apparatus could be also used as an annealing furnace. As the specimen and thermometer were situated within the heating coil they could be very rapidly brought up to the desired temperature, which was read direct on a Callendar-Griffiths platinum pyrometer. Regulation was effected by alterations in the grouping of the coils as well as by outside resistances. The measurements of hysteresis were made as before with Professor Ewing's hysteresis tester, the specimens being removed periodically from the oven and tested at atmospheric temperature.

A number of short runs at various high temperatures were taken. The results given in Table I and fig. 1 represent the means of several independent observations at each temperature.

The absolute values of the hysteresis are given in ergs per cubic centimetre per cycle (for $B = 4000$), together with the rise expressed as a percentage of the initial hysteresis to the nearest 1 per cent. The general features of the action are similar to those noticed before at more moderate temperatures. They should be compared with fig. 2 of the former paper. The initial rise of hysteresis is more rapid the higher the temperature, but the subsequent fall takes place sooner, and the final state is one of lower hysteresis the higher the temperature, until at about 700° C., a temperature just short of that required for complete annealing, the hysteresis falls again to quite its original value

* 'Roy. Soc. Proc.,' vol. 63, pp. 258—267.



after a very short time. At temperatures above this, no trace of any rise has been observed.

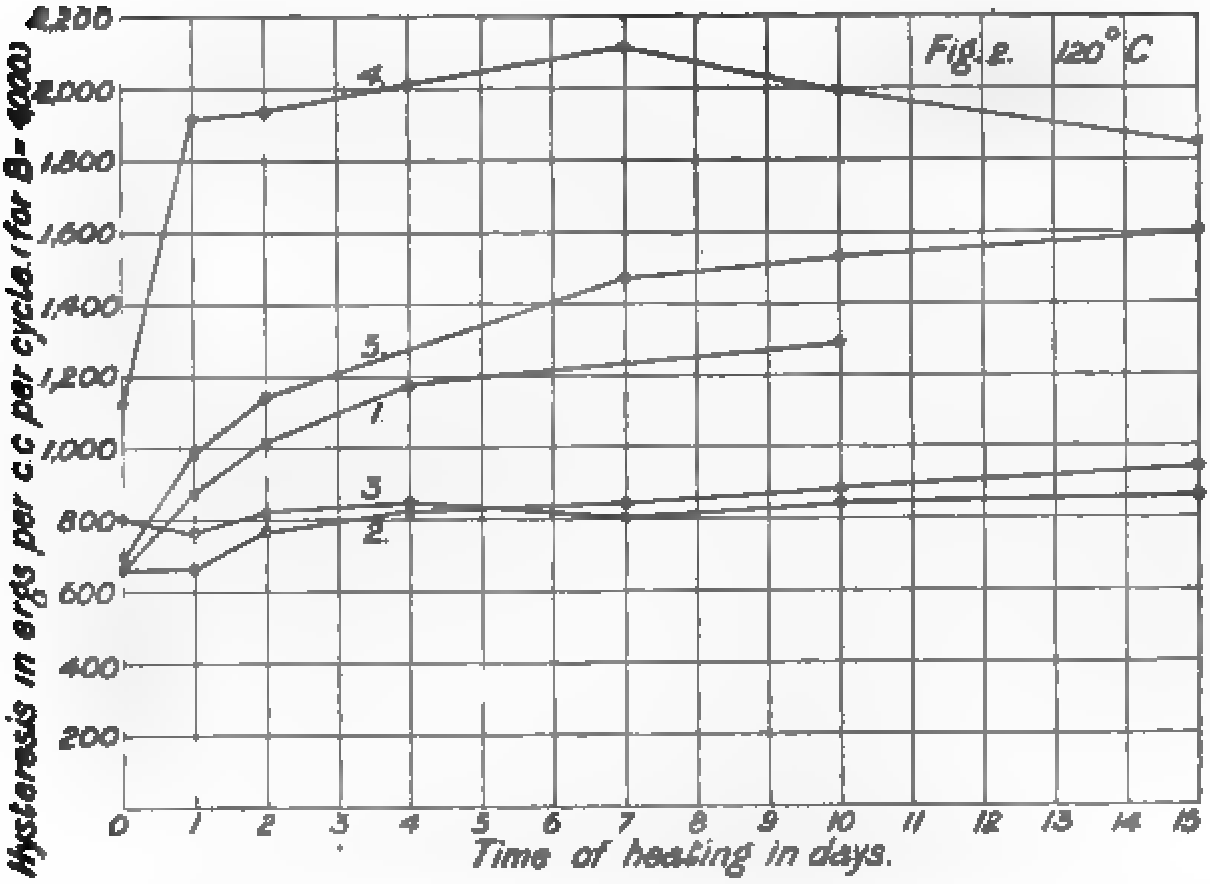
The question at once suggested itself whether iron which had been heated at a constant temperature with the effect of causing its hysteresis to pass a maximum and to become reduced, would have its hysteresis again increased by exposure to more moderate temperatures,

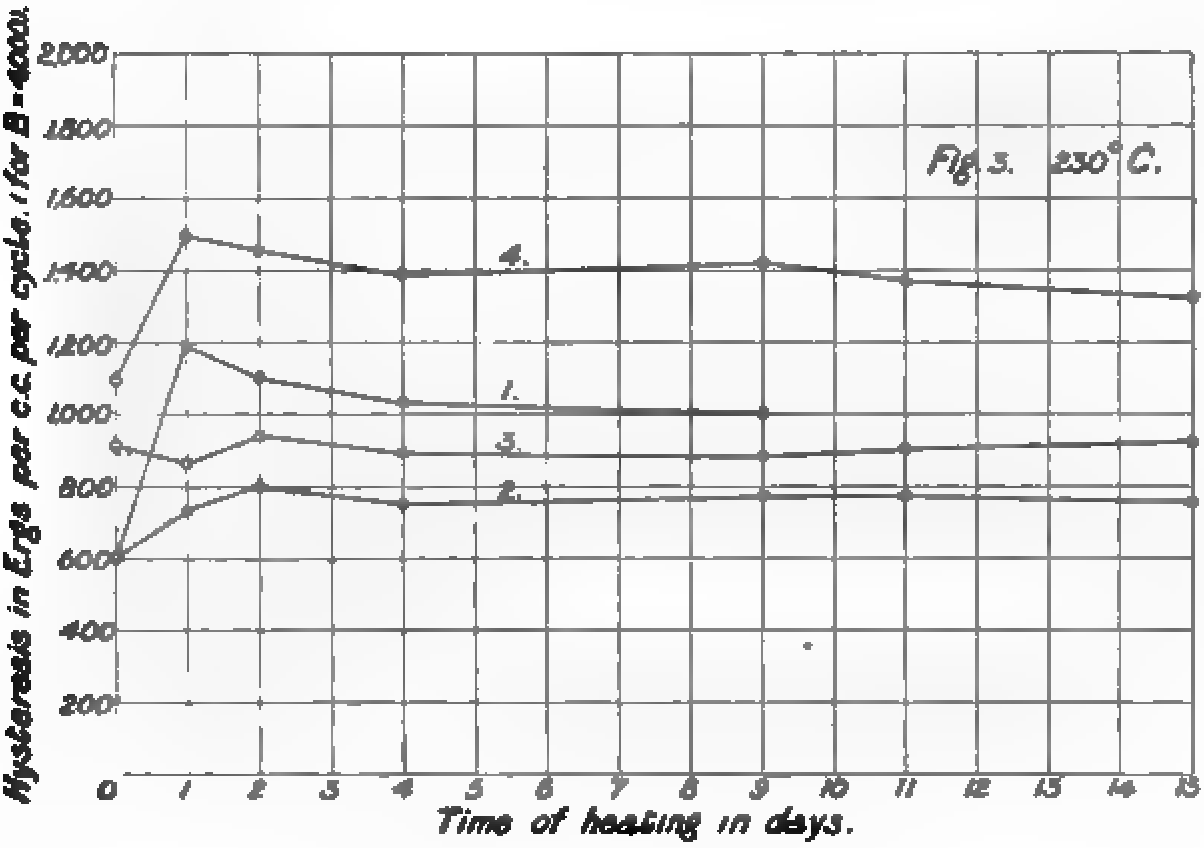
or would show some degree of immunity to temperature effects. It was found that the susceptibility to change at moderate temperatures was not by any means entirely removed by prolonged heating at high temperatures. The subsequent action of low temperatures was, however, slower than in freshly annealed iron, especially after the high temperature had been applied for a considerable time, also it appeared that temperatures at 500° or 600° C. produced less effect on the susceptibility to subsequent change at lower temperatures than was produced by more moderate degrees of preliminary heating. A complete series of experiments on this point, however, has not yet been made, only a few samples having been re-heated in this way, but the results obtained with these were consistent, and pointed to the above conclusions.

The latter part of the action at the higher temperatures resembles an incomplete annealing, as there appears to be little difference between the state after the iron has been heated for a few hours at a temperature just short of the "critical" temperature at which ferromagnetic quality disappears, and that of the same material which has been heated above this temperature so as to become completely annealed.

The above experiments, and those in my previous paper, refer to one particular brand of iron, all the specimens having come from the same sheet. A few examples, showing how widely different is the behaviour of different brands of steel and iron, are given in figs. 2 and 3. The data for these diagrams are to be found in Tables II and III, where the absolute values of the hysteresis in ergs per cubic centimetres per cycle are given together with the rise expressed as a percentage of the initial hysteresis.

Figs. 2 and 3 relate to various samples of commercial iron and steel, some of which were supplied by makers in this country and some from America. Fig. 2 shows the effects of heating at 120° C., and fig. 3 shows the effects of heating specimens of the same iron at 230° C. The curves, numbered alike in both figures, refer to the same material. All these samples were initially in the annealed state. No. 1 is a piece of the iron used in the previous experiments, and is given here for the sake of comparison. No. 2 is a sample of special transformer steel of equally low initial hysteresis. The action of heat on it is similar in general characteristics to the action of No. 1, but much less in degree. No. 3 is practically "non-ageing," even in the annealed state, and although not of such low initial hysteresis as some of the other specimens, would be the most suitable for transformers on account of its immunity from change by prolonged heating. No. 4 is a sample of sheet-iron not specially made for transformers; it is of poor magnetic quality, but is interesting as showing, at 120° C., effects which require a higher temperature in the other brands of iron; the initial rise is very rapid, and the subsequent fall of hysteresis is clearly shown, even at





this temperature. No. 5 shows greater rise than No. 1 at 120° C. in the annealed state, although this identical sample appeared to be practically unaffected by heating for a fortnight in the state in which it was supplied, but was then of somewhat higher initial hysteresis. It is interesting to notice that this iron if annealed before use in a transformer would ultimately, through low temperature heating, show much

more hysteresis than if left in the state in which it was submitted by the manufacturer.

The author has no information as to the treatment by which this remarkable degree of "non-ageing" quality had been produced. The general characteristics of the action at 230° C. on the different samples (see fig. 3) are much the same, differing only in degree. No. 3 is, again, little changed by prolonged heating.

It seems from these and other tests, that brands of transformer steel, which are practically "non-ageing," are obtainable commercially, but they are not (at least in these examples) of such low initial hysteresis as the "Swedish iron," which was formerly considered the best material for transformers. The effects of annealing vary much in different samples. All the samples tested after annealing have been found to be more liable to change in that state than in the state in which they were supplied by the makers. The method of annealing and rate of cooling may have much to do with the "non-ageing" quality of the material. Incidentally the experiments have given some evidence that samples of iron may undergo a slight change in hysteresis, even if kept at atmospheric temperature for three or four years.

It may be convenient to briefly summarise the chief effects of prolonged heating on the magnetic properties of iron which have been observed in these and the previous experiments.

1. Material in the annealed state is more liable to change than in a harder state.

2. All the changes produced by prolonged heating are completely removed by re-annealing.

3. The heating need not be continuous; the same cumulative effect is produced by a number of short periods at a given temperature as by a continuous heating at the same temperature.

4. The effect may be regarded as being due to two actions superposed, one tending to increase the hysteresis, this action being the more prominent at lower temperatures; the other analogous to an incomplete annealing, tending to decrease the hysteresis, this action predominating at higher temperatures.

5. The liability of the material to increase in hysteresis at moderate temperatures is not removed by prolonged heating at high temperatures.

6. The change is confined to the lower part of the B.-H. curve, the saturation value of the magnetisation being substantially unaltered.

7. The effect is produced equally, whether the iron is or is not exposed to the air during heating.

In conclusion the author wishes to express his thanks to Professor Ewing, for placing at his disposal the facilities which have enabled these experiments to be carried out, and for much other kind help.

Table I.—Change of Hysteresis by Prolonged Heating at High Temperatures.

Temp.	300° C.		400° C.		500° C.		600° C.		700° C.	
Time of heating.	Hysteresis.		Hysteresis.		Hysteresis.		Hysteresis.		Hysteresis.	
	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.
0	560	0	590	0	620	0	590	0	640	0
3 m.	750	34	1040	77	990	59	860	46	690	8
7 „	1160	107	900	53	940	52	750	27	650	1·5
15 „	1010	80	850	44	810	31	710	20	650	1·5
30 „	950	70	820	39	780	26	700	19	650	1·5
1 hr.	950	70	800	36	780	26	640	9	640	0
2 „	940	68	800	36	760	23	640	9	640	0
3 „	940	68	800	36	740	19	640	9		
5 „	940	68	800	36	730	18				
1 day	860	54	790	34	730	18				

Table II.—Various Brands of Iron and Steel heated at 120° C.

No. of specimen.	1.		2.		3.		4.		5.	
Time of heating, days.	Hysteresis.		Hysteresis.		Hysteresis.		Hysteresis.		Hysteresis.	
	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.
0	660	0	660	0	800	0	1120	0	690	0
1	870	32	660	0	760	−5	1910	71	980	42
2	1010	53	760	15	810	1	1930	72	1140	51
4	1170	77	820	24	840	5	2010	80	..	..
7	..	..	840	27	800	0	2110	89	1470	113
10	1290	96	880	33	820	2	1990	78	1530	122
15	..	..	940	42	830	4	1840	64	1600	132

Table III.—Various Brands of Iron and Steel heated at 230° C.

No. of specimen ...	1.		2.		3.		4.	
Time of heating, days.	Hysteresis.		Hysteresis.		Hysteresis.		Hysteresis.	
	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.	Abs.	Incr. per cent.
0	600	0	600	0	910	0	1090	0
1	1190	98	730	22	860	−8	1490	37
2	1100	77	800	33	940	3	1450	33
4	1030	72	750	25	890	2	1390	27
9	1000	67	770	28	880	3	1420	28
11	..	..	770	28	900	1	1370	24
15	..	..	750	25	910	0	1320	19

"On the Topographical Anatomy of the Abdominal Viscera, especially the Gastro-Intestinal Canal in Man." By CHRISTOPHER ADDISON, M.D., B.S. (Lond.), F.R.C.S., Professor of Anatomy, University College, Sheffield. Communicated by Professor ALEXANDER MACALISTER, F.R.S. Received October 15—Read December 8, 1898.

(Abstract.)

General Purpose.

This paper embodies the results of an enquiry into the topographical anatomy of the abdominal viscera in man. The work falls into two main parts. First, that dealing with the relations of the viscera to the surface of the body; and, second, that dealing with the relations of the viscera to one another.

With regard to the first part: It is to be remarked that the methods of mapping out the abdomen at present in general use are open to certain objections; for the reasons that the lines used to divide the abdomen transversely are drawn at variable distances from one another, the variation not being determined by the dimensions of the body; that the points between which the upper transverse abdominal line is drawn are very variable in their level, so that in some cases the transverse lines come very near together leaving a large part of the abdomen above them not mapped out; in other cases the lines may be far apart; and, moreover, the points between which the upper transverse line is to be drawn are not always easily determined, and it happens

that in many cases the line, if drawn correctly, is not horizontal. For these reasons the transverse lines as fair and uniform divisions of the abdomen, and as guides to the deeper parts, lose very much value. Further it is desirable for practicable purposes that the determination of the positions of parts of the viscera in regard to the surface by measured distances, in centimetres or inches, as so much above, below, to the right or left of certain points or lines is to be avoided as much as possible, seeing the very great variations that occur in the dimensions of the trunk even in adults.

The author has therefore sought to elaborate first some method of abdominal surface-marking which shall be independent of the variable surface points, which shall be unvarying and uniformly proportionate to the size of the trunk, which shall be easily determined and in which the lines used to divide the abdomen shall be possessed of such constancy both in regard to the bony skeleton and the viscera that they themselves become reliable land-marks.

With regard to the second part of this enquiry: Many very accurate measurements of the position of individual organs or parts with regard to the surface of the body are recorded. But it is of great importance that, in any one case, not only the position of any one organ should be recorded, but that of all the other organs or parts in its neighbourhood, for in this way only can we discover the degree of interdependence in the positions of the various organs, and the extent, if any, to which when they are enlarged, or diminished in size, or displaced, they tend to cause alterations in the positions of the various neighbouring organs; and, from the clinical point of view, it is perhaps as important to determine that changes in the shape or position of any one organ do not tend to cause alterations in the position of any other particular organ, as to determine that they do. In the second part of this enquiry therefore, by studying and comparing a series of cases, the author has endeavoured to determine the forces which maintain the various mutual relations of the abdominal viscera or which cause alterations in shape or displacements of them either as a whole or with regard to one another. And in this connection has been considered, in the same manner, the position, and changes in position, of the various lines of the peritoneal attachments to the body wall.

Method.

For obtaining the maps of the viscera the bodies of forty subjects, taken consecutively, were examined in the fresh state, with the exception of two bodies that were hardened before examination by fluid injections. In all but a few cases, the examination was within thirty hours after death. The examination was conducted in the following manner:—Tables were prepared in each case recording the stature of the individual,

the general condition, the cause of death, the dimensions of the trunk in various directions, the distances from one another of the various bony and other surface points and their relations to the lines used to divide the abdomen, and various other general and particular facts. At the end of the examination the relations of the various surface lines to the vertebral column and the parts at the back of the abdomen were measured and recorded, as will be explained. These tables are presented in an appendix as the *Individual Case Tables.*

The different bony and other surface points, the parts of the costal arch, and the lines used to divide the abdomen in their appropriate positions were then drawn on the life-size scale on large sheets ruled in centimetre squares.

The abdomen was divided vertically by three lines, a middle line and two lateral lines, one drawn upwards on each side through a point midway between the anterior superior spine of the ilium and the middle line. Lines were drawn transversely across the trunk through points a quarter-way, half-way, and three-quarters of the way along a tape drawn from the pubes to the supra-sternal notch. The lower two transverse lines were abdominal. Steel pins 14 inches long, sufficiently thick, so as to ensure rigidity, and with long well-sharpened points, were then hammered through the abdomen at right angles to the table, into which they were driven when they failed to fasten themselves in the bony skeleton. Six pins were driven through the abdomen; three in each transverse plane, one on the middle line and one in each lateral line.

The anterior abdominal wall was then cut free of the pins and reflected so as to completely expose the parts beneath.

The various viscera and other parts were then measured in relation to the pins in various directions at the point of transfixion, and a life-size outline of them made on the ruled sheets. No parts were disturbed before measurement, and they were cut away piecemeal, as required, to expose the parts beneath. In this way, at length, a complete map of all the viscera was obtained in relation to the pins projecting the surface-marking through the abdomen. The same applies to the lines of the peritoneal attachments. Subsidiary drawings were made, as might be required, of peritoneal pouches or other parts, and of the viscera from different aspects. At the end of the examination the relations of the pins to the parts of the skeleton behind and to the brim of the pelvis were recorded and drawn. A map of the viscera was in this way obtained both in relation to the surface lines and to the different bony and other surface points, so that if the method of dividing the abdomen had been found unsatisfactory, the measurements could be transferred to any other system. Outlines of all the viscera and the chief surface points from each case are represented on *one sheet on the life*-size scale in the *Case Plates* of the Appendix. All

the individual separated viscera are represented, grouped along the various planes used to divide the abdomen, on the one-ninth scale in different parts of the paper; and in various other manners by means of diagrams, curves, and tables their correspondence and variations with regard to the surface and to one another are set forth. Finally, full details of all the measurements of the viscera, and their averages, are given in the *Measurement Tables* in the Appendix.

Various hardened, fœtal, and adult preparations have been made to elucidate or illustrate different points in the paper.

In the course of the paper each organ is considered somewhat as follows:—Its average position in regard to the surface; its variations in regard to the surface; its average position in relation to other viscera; its different variations in regard to them; the causes of its variations; its shape and movements.

Results.

The value of the method of the proportionate division of the abdomen, already described, for the purposes of surface marking is well established. Indicating some of the chief points—

First, concerning the upper transverse abdominal line, halfway between the pubes and the supra-sternal notch:—

It is found to practically correspond with the disc between the first and second lumbar vertebræ—in 67·5 per cent. of the cases it was within half an inch of this disc; its greatest distance from the disc, and that only in one case, was 1 inch. It corresponded in the average at the costal arch with the tip of the 9th costal cartilage; but it is superior to that part for surface-marking purposes because (*a*) its vertebral variation was not so great, and (*b*) it can always readily be obtained, whilst the tip of the cartilage in many subjects cannot be localised.

In regard to deeper parts, a horizontal plane at this level (*a*) practically bisects the stomach as it overlies the middle line in the average of cases.

(*b*) In 72·5 per cent. of the cases it was correct as a guide to the level of the pylorus; in these cases either passing through the pylorus or corresponding with one of its borders.

(*c*) In the right lateral line it represents the place where the gall bladder overlies the duodenum, and, when the liver is not enlarged or displaced downwards, the plane passes just above the highest point of the hepatic flexure of the colon.

(*d*) To the left of the middle line, nearly halfway between the middle line and the lateral line, it indicates the highest point of the duodeno-jejunal flexure and the upper border of the mesentery, which in 85 per cent. of the cases were not situated more than 2 cm. away from the plane one way or the other.

(*e*) It almost invariably crosses some part of the head of the pancreas, usually about its upper third, and in the left lateral line it represents, in the average, the anterior border of the pancreas, this part being, in 70 per cent. of the cases, at or within 2 cm. of the plane.

(*f*) In the left lateral line it represents, in the absence of a distended stomach, the anterior border of the pancreas, the greater curvature of the stomach, the attachment of the transverse meso-colon, and the upper border of the transverse colon.

(*g*) Further to the left beneath the ribs it represents the upper part of the basal surface of the spleen.

Second, concerning the *lower transverse abdominal line*, quarterway from the pubes to the supra-sternal notch :—

It occupies a very regular position with regard to the ilium, representing practically Cunningham's intertubercular plane. It is normally situated 2 inches above the anterior superior iliac spines, and is found, in regard to these points and the highest parts of the iliac crest, to be somewhat less variable than the umbilicus. In regard to vertebræ, it is situated over the upper part of the fifth lumbar vertebra.

In regard to deeper parts :—(*a*) It represents the place where the psoas muscles diverge from the lumbo-sacral promontory, and passes a little above the inner attachment of the meso-sigmoid.

(*b*) In the right lateral line it represents the upper border of the ileo-colic junction—the inner border of the ascending colon at this point being situated immediately external to the lateral line.

(*c*) In the left lateral line it passes a little above the commencement of the meso-sigmoid—the inner border of the descending colon at this point being situated immediately external to the lateral line.

Third, taking a plane across the abdomen midway between the transverse lines :—For practical purposes it represents in each lateral line the lower pole of the kidney, passing a little above that of the right and a little below that of the left; in the right lateral line it indicates the turning inwards of the peritoneum to form the commencement of the transverse meso-colon; and in the middle line the crossing of the mesentery.

Fourth, taking a plane midway between the lower transverse abdominal line and one through the anterior superior iliac spines, it represents in the right lateral line the root of the appendix, and a little internal to this, at the pelvic brim, the lower attachment of the mesentery and the innermost point of the cæcum.

This aspect of the paper, however, need not be further enlarged upon. An indication has been given of the position of some of the more important points of various parts around which others may be easily filled in. Suffice it to say, that, as with regard to the surface lines, so the levels and variations of the different viscera with regard to the more stable surface points, such as the parts of the ilium and

the infra-sternal notch, as well with regard to the more variable parts of the costal arch and the umbilicus, were fully worked out, and are set forth in the paper.

Further, in regard to deeper parts and the various visceral displacements :—

1. *In connection with the stomach*—(*a*) It is found that a low position of the stomach, even combined with distension, is not sufficient to cause material downward displacement of the pylorus, but that that part, firmly bound with the first part of the duodenum to the liver requires downward displacement or enlargement of the liver—particularly of its omental tuberosity—for it to be substantially moved downwards.

(*b*) Lateral displacements of the pylorus also are found more related to the condition of the liver than to that of the stomach; and the evidence does not point to any considerable displacement of the pylorus to the right in the filling of the stomach. Similarly, displacements of the duodenum and the head of the pancreas to the left are associated with a low position of the lower border of the liver.

(*c*) Concerning the "stomach-bed" described by Birmingham, the parts behind the stomach vary with the condition of the stomach in this manner :—When the stomach is distended or situated low down, it flattens out the pancreas, increasing the vertical extent of its gastric surface and diminishing the prominence of its anterior border and the depth, antero-posteriorly, of its inferior surface; and the pancreas is further pushed down over the face of the left kidney, leaving an increased gastric surface of that organ exposed above its upper border. The reverse of all these processes takes place when the stomach is pushed upwards by distended intestines below.

(*d*) The stomach does not displace the left kidney downwards; in fact the position of the left kidney is not found to vary directly with that of any other organ in its neighbourhood, but is chiefly dependent for its maintenance upon the strength of its enveloping connective tissue.

It may here be mentioned, that the level of the left supra-renal body in relation to the left kidney is determined very much by the level of the pancreas with regard to the kidney. When the pancreas is pushed down over the kidney the supra-renal body follows it, but is not depressed to so great an extent; and the reverse takes place when the pancreas is pushed upwards.

2. The duodenum and the head of the pancreas have a considerable range of level compared with the vertebral column—as great as, or greater than, that of the right kidney. These alterations in level of the duodenum and the head of the pancreas are found to be chiefly related to the position and size of the liver—which, on the other hand, does not appear to be potent to displace the right kidney downwards.

3. Although the ascending colon usually makes a considerable impression on the right kidney, yet that part of the bowel is not an indispensable support of the right kidney; the bowel may be displaced away from a right kidney situated at a level, as high, or higher than usual. The right kidney is chiefly maintained in its position by the strong attachments of its enveloping connective tissue, particularly to the right crus of the diaphragm.

4. Prolapse of the mesentery is commonly associated with prolapse of the splenic flexure of the colon, but more directly associated with the condition of the liver and stomach, as far as the forces above it are concerned.

The costo-colic ligament is the chief agent in determining the position of the splenic flexure of the colon, and, though commonly giving way before a liver and stomach displaced or enlarged downwards, may maintain the position of the splenic flexure of the colon in spite of them.

5. Although in the fœtus the arrangements of the coils of the small intestine perhaps generally follow certain plans, as far as these cases go, the coils do not appear to maintain these arrangements in the adult with any special uniformity.

Many other points in the paper of importance do not admit of being explained or indicated in an abstract; they are especially the parts dealing with the variations in the level of the cardiac orifice of the stomach, and the relative levels of the two orifices of the stomach—the varieties in shape of the stomach, how that there appear to be four chief types, and that the first and commonest is particularly noticed in those cases in which the transverse colon occupies a high position—the relations of the stomach to the liver—the shape and moulding of the pancreas by the stomach, and how that the presence of a well-defined omental tuberosity on the pancreas is associated with a distended and low position of the stomach, not especially with distension only—the abnormalities of the duodenum as illustrated in these cases—the position, direction, and moveability of the lower end of the ileum—the peritoneum on the large intestine—the classification of the position and attachments of the vermiform appendix, the changes in its position with regard to the cæcum, and the associated conditions; and the changes in the position of the cæcum itself and the associated conditions—the varieties in shape of the transverse colon; prolapse of the transverse colon and the associated conditions—the description of the meso-sigmoid, especially the length and attachments of its outer limb, and the resulting condition of the upper part of the sigmoid flexure of the colon.

"Mathematical Contributions to the Theory of Evolution. VI. Reproductive or Genetic Selection. Part I. Theoretical." By KARL PEARSON, F.R.S. "Part II. On the Inheritance of Fertility in Man." By KARL PEARSON, F.R.S., and ALICE LEE. "Part III. On the Inheritance of Fecundity in Thoroughbred Race-horses." By KARL PEARSON, F.R.S., with the assistance of LESLIE BRAMLEY-MOORE. Received November 14—Read December 8, 1898.

(Abstract.)

1. The object of this memoir is twofold: first, to develop the theory of reproductive or genetic selection* on the assumption that fertility and fecundity may be heritable characters; and, secondly, to demonstrate from two concrete examples that fertility and fecundity actually are inherited.

The problem of whether fertility is or is not inherited is one of very far reaching consequences. It stands on an entirely different footing to the question of inheritance of other characters. That any other organ or character is inherited, provided that inheritance is not stronger for one value of the organ or character than another, is perfectly consistent with the organic stability of a community of individuals. That fertility should be inherited is not consistent with the stability of such a community, unless there be a differential death-rate, more intense for the offspring of the more fertile, *i.e.*, unless natural selection or other factor of evolution holds reproductive selection in check. The inheritance of fertility and the correlation of fertility with other characters are principles momentous in their results for our conceptions of evolution; they mark a continual tendency in a race to progress in a definite direction, unless equilibrium be maintained by any other equipollent factors, exhibited in the form of a differential death-rate on the most fertile. Such a differential death-rate probably exists in wild life, at any rate until the environment changes and the equilibrium between natural and reproductive selection is upset. How far it exists in civilized communities of mankind is another and more difficult problem, which I have partially dealt with elsewhere.† At any rate it becomes necessary for the biologist either to affirm or deny the two principles stated above. If

* I have retained the term "reproductive" selection here, although objection has been raised to it, because it has been used in the earlier memoirs of this series. Mr. Galton has kindly provided me with "genetic" and "proliferal" selection. The term is used to describe the selection of predominant types owing to the different grades of reproductivity being inherited, and without the influence of a differential death-rate.

† Essay on Reproductive Selection in 'The Chances of Death and other Studies in Evolution,' vol. 1, p. 63.

he affirms them, then he must look upon all races as tending to progress in definite directions—not necessarily one, but possibly several different directions, according to the characters with which fertility may be correlated—the moment natural selection is suspended; the organism carries in itself, in virtue of the laws of inheritance and the correlation of its characters, a tendency to progressive change. If, on the other hand, the biologist denies these principles, then he must be prepared to meet the weight of evidence in favour of the inheritance of fertility and fecundity contained in Parts II and III of the present memoir.

2. The theory discussed in Part I opens with the proof that if fertility be a function of any physical characters which are themselves inherited according to the law of ancestral heredity, then it must itself be inherited according to that law. As fertility would certainly appear to be associated with physique, we have thus an *à priori* argument in favour of its inheritance.

3. In the next place the influence of "record" making on apparent fertility is considered. The mother with more offspring has a greater chance than one with fewer of getting into the record which extends over several generations, and, further, if every possible entry be taken from the record, she is again weighted with her fertility. Thus a record is not a true account of the fertility of successive generations. The fertility of mothers is always found to be more and their variability less than the fertility and variability of daughters. Accordingly from the apparent fertility and variability of the record the actual values in each generation must be deduced. The difficulties and the theory of this investigation are developed at some length, and methods determined by which it can be ascertained whether a secular change in fertility is actually taking place. The results obtained are extended to fecundity.

4. In the case of thoroughbred horses, their number is so few and in-breeding so great owing to the fashion in sires and stocks, that we have to deal with a large array of offspring of the same sire. It is easy accordingly to obtain 50,000 to 150,000 pairs of a given relationship, *e.g.*, half-sisters, and we rapidly get numbers too large for forming correlation tables in the usual manner. Accordingly methods are developed for finding correlation coefficients from the means of "arrays." These methods are of considerable importance, for they enable us to ascertain the correlation between a latent character in one sex and a patent character in another, or between characters latent in two individuals. Thus, it is shown that the correlation between the brood-mare's fecundity latent in two related stallions can be deduced from the correlation between the mean fecundities of their two arrays of daughters. In this way a numerical estimate can be formed of the *inheritance of latent* characters.

5. The brood-mare is for many causes, detailed at length in the paper, a highly artificial product, and accordingly the record gives a considerable percentage of fictitious fecundities. The effect of a mixture of correlated and uncorrelated material on correlation and variation is next investigated, and it is shown that the former is more seriously affected than the latter. Hence results based on variation are more likely to be trustworthy than those which use correlation. Incidently the problem of the mixture of heterogeneous materials uncorrelated in themselves is investigated, and it is shown that a correlation will result in the mixture. This *spurious* correlation is of some importance for the question of mixtures of classes in fertility problems, but it is also significant of the general danger of heterogeneity in bio-statistical investigations, and further indicative of the possibility of creating correlation between two characters by breeding between small heterogeneous groups in which this correlation is zero. This illustration suffices to indicate how correlation between characters does not necessarily indicate a causal relationship.

6. Part II of the memoir deals with the inheritance of fertility in man. It is first shown by large numbers that fertility is undoubtedly inherited from mother to daughter, but that if we include all types of marriages the inheritance is largely screened by other factors. An attempt is made to remove one by one these factors, and the more stringently this is done the more nearly the regression of daughter on mother moves up towards the value required by the law of ancestral heredity. If we could take only marriages in which both daughter and mother were married during the whole of their fecund period there is little doubt that we should find inheritance according to the law of ancestral heredity. The sparseness of homogeneous material hinders, however, such an investigation.

The inheritance of fertility from father to son is then considered; this is really rather an inheritance of sterility or tendency to sterility, for the full fecundity of a man is not usually exhibited in monogamic union. It is rather a problem of whether his fecundity lasts as long as his wife's. We find definite inheritance of this sterile tendency from father to son, although for the reason just given it falls below that indicated by the law of ancestral heredity.

Lastly, the inheritance of fertility in the woman through the male line is dealt with, and it is shown that a woman's fertility is as highly correlated with that of her paternal as with that of her maternal grandmother. In other words the latent character, fertility in the woman, is transmitted through the male line, and with an intensity which approximates to that required by the law of ancestral heredity. Incidentally the problem of "heiresses" is dealt with. It is shown that in the case of women who are chiefly "heiresses," there is at once a considerable drop in the correlation between their fertility and that of

their mothers, while there is a small drop only in their average fertility. In other words, an "heiress" is not to be looked upon as coming in general from a sterile stock, but as having a mother, whose fertility has a fictitious value, *i.e.*, the apparent fertility of the record is not the potential fertility, the inherited character, in the mother. In other words "heiresses" are not as a rule due to sterile mothers, but in the bulk are due to such causes as late marriages, restraint, incompatibility of husband and wife, absence of sons or death of other children, &c., &c.

7. Part III of the memoir contains the results of a somewhat laborious investigation into the fecundity of brood-mares, which has been a number of years in progress. Had better material been available for the inheritance of fecundity, we would gladly have adopted it in preference to dealing with such an intricate subject as the breeding of race-horses. Unfortunately the absence of place and means hindered any experimental investigation on our part into the inheritance of fecundity in some simpler type of life. Such investigation ought certainly to be made by a trained biologist with the knowledge and the laboratory at his disposal.

After discussing at length the steps taken by us to measure and tabulate the fecundity of brood-mares, we deduce the following conclusions :—

(i.) Fecundity in the brood-mare is inherited from dam to mare.
(ii.) It is also inherited from grand-dam to mare through the dam.

In both these cases the intensity is much less than would be indicated by the law of ancestral heredity, but the divergence is not such that it could not be accounted for by a percentage of fictitious values such as the peculiar conditions of horse-breeding warrant us in considering probable.

(iii.) The latent quality, fecundity in the brood-mare, is inherited through the sire; this is shown not only by the correlation between half-sisters, but by actual determination of the correlation between the latent character in the sire and the patent character in the daughter.
(iv.) The latent quality, fecundity in the brood-mare, is inherited by the stallion from his sire. This is shown not only by the fecundity correlation between a sire's daughters and his half sisters, but also by a direct determination of the correlation between the latent quality in the stallion and in his sire.

In both these cases of latent qualities the law of inheritance approaches much more closely to that required by the Galtonian rule This is probably due to the fact that the determination of the correlation is thrown back on the calculation of the means and variabilities of

arrays, and not on the direct calculation of the correlation between fecundities, a large percentage of which are probably fictitious (see § 5).

8. Parts II and III accordingly force us to the conclusion that fertility is inherited in man and fecundity in the horse, and therefore probably that both these characters are inherited in all types of life. It would indeed be difficult to explain by evolution the great variety of values these characters take in allied species, if this were not true. That they are inherited according to the Galtonian rule seems to us very probable but not demonstrated to certainty. It is a reasonable hypothesis until more data are forthcoming.

The memoir concludes with a discussion of the meaning of reproductive selection for the problem of evolution and with sixteen correlation tables, giving the dressed material on which our conclusions are based.

"'Nitragin' and the Nodules of Leguminous Plants." By MARIA DAWSON, B.Sc. (Lond. and Wales). Communicated by Professor H. MARSHALL WARD, F.R.S. Received November 19,—Read December 8, 1898.

(Abstract.)

A study of the nodules found upon the roots of leguminous plants has led the author to an unhesitating confirmation of the parasitic nature of both the filaments and the bacteroids contained in these organs. The filaments, it was found, have no such constant relation to the nucleus of the cells, as was represented by Beyerinck in 1888. By plasmolysis of the root-hairs, the infection tube is shown to have grown into the hair, and not to correspond with the primordial utricle of the hair, a result which proves that Frank was mistaken in regarding the tube as formed from the contents of the hair mingled with fungal protoplasm. By staining with aniline blue and orseillin these tubes and the filaments in the cells were shown to consist of strands of straight rodlets, lying parallel to the longer axis of the filament, and embedded in a colourless matrix. This matrix does not consist of cellulose, chitin, or any form of slime. The swellings upon the filaments occur at places where the rodlets have become heaped up, and at such places the filaments eventually burst, liberating the rodlets, whilst they themselves remain as pointed portions, directed towards each other in the cells. After liberation from the filaments, the rodlets become transformed into X, V, and Y-shaped bacteroids. This variety of shape does not occur when these organisms are cultivated outside the plant on a solid medium, but in liquid pea extract the change from straight rodlets to "bacteroids" occurs in a few days. By cultivating these organisms in drop cultures under constant observa-

tion with high powers, these rodlets are seen to multiply by division into equal, or sometimes slightly unequal, halves. By this method the author hopes also to determine whether the change in shape arises from fusion of two or more individuals or by branching. Their multiplication by division leads to the conclusion that these organisms are members of the Schizomycetes; whether or not they are true Bacteria must, however, still be undecided, until the final stage in their life history has been fully followed.

The X, V, or Y-shaped bacteroid, when once formed appears to be incapable of further growth. These organisms are aërobic in character, their power of fixing atmospheric nitrogen is to be tested in connection with their growth on silicic acid gelatine. Commercial "Nitragin" consists of minute micrococcus-like bodies, all straight and immobile. They multiply rapidly on gelatine media, and in pea extract become converted into "bacteroids" as well as straight rods. Nitragin does consist of the tubercle organism, and as a result of the inoculation of either seeds or soil with it, tubercle formation takes place. Crossing of kinds supplied for different genera and species is quite successful within the tribe Vicieæ. In order to test the possibility and conditions of direct infection of the roots, seedling peas, starting both before and after germination, were grown in sterile tubes, by which means the whole plant was kept under control. This method showed that direct infection of quite young radicles is tolerably certain, also of older roots, provided the conditions under which germination occurred are maintained after infection.

In order to secure infection it is not necessary that the organism should pass through the soil, and the age of the root-hair at the time of infection seems to be without effect upon the result. An accumulation of CO_2 round the roots is not the cause of failure in direct infection.

The addition of nitragin to soils rich in nitrates appears to be inadvisable, but a supply of it to soil poor in nitrates results in an increased yield, though better results are obtained if instead of nitragin, nitrates be added to the soil.

"Preliminary Note on the Spectrum of the Corona." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received November 11—Read November 24, 1898.

(PLATE 4.)

The announcement by Professor Nasini of the possible presence of the characteristic green line of the corona in the spectrum of the gases collected at the solfatara of Pozzuoli,* renders it desirable that I should at once publish some of the results of an investigation relating to the spectrum of the corona with which I have lately been occupied.

* 'Nature,' vol. 58, p. 269, July 21, 1898.

In the course of my early observations of the spectrum of the chromosphere, I discovered on June 6, 1869, a bright line at 1474 on Kirchhoff's scale, which I stated to be coincident with a line of iron.*

During the total eclipse of the sun on August 7, 1869, a green line was recognised by Professor Young as belonging to the spectrum of the corona, and the position of this line was also stated to be 1474K.

Although other determinations of the position of the green line of the corona during eclipses have not all agreed absolutely with Young's observations, the differences have been attributed to errors of observation, so that Young's statement of the coincidence of the coronal and chromospheric lines, and their correspondence with the solar dark line at 1474K has been generally accepted. No special attention appears to have been directed of late years to the measurement of the corona line itself.

This and other coronal radiations were photographed as rings by the use of prismatic cameras in 1893, 1896, and 1898, but a full list of them has only so far been published for the photographs taken by Mr. Fowler during the eclipse of 1893.† Among the brightest of these rings, which is common to all three sets of photographs, is one about wave-length 4231, which probably is identical with the corona line photographed by Schuster in 1886, and stated to have a wave-length of 4232·8 on Ångström's scale (4233·4 Rowland). Schuster stated that this line was "probably the same line as 4233·0 often observed by Young in the chromosphere."‡ The chromospheric line at this wave-length has since been identified as an enhanced line of iron, of which the precise wave-length is 4233·3. Captain Hills photographed this corona line with a slit spectroscope in the last eclipse, and he gives its wave-length as 4233·5,§ which within the limits of error might be considered coincident with the enhanced line of iron.

The later researches on the spectrum of iron have shown that the iron line which I observed in 1869 to be coincident with the bright chromospheric line at 1474K (5316·79 Rowland) is also an enhanced line, agreeing absolutely with Young's latest determination of the wave-length of the 1474 chromospheric line,|| with which, according to his eclipse observations, the green line of the corona is coincident.

According to these results then, two of the chief lines in the spectrum of the corona would be coincident with enhanced lines of iron. The remaining corona lines, which have so far been measured, are not, however, coincident with enhanced lines. It did not seem

* 'Roy. Soc. Proc.,' vol. 18, p. 76.

† 'Phil. Trans.,' A, vol. 187, p. 593.

‡ 'Phil. Trans.,' A, vol. 180, p. 341.

§ 'Roy. Soc. Proc.,' vol. 64, p. 54.

|| Scheiner's 'Astronomical Spectroscopy' (Frost's translation), p. 425.

possible, therefore, that two of the enhanced lines of iron should be present without the others, even if it be admitted that the corona may have a temperature high enough to produce any enhanced lines.

It appeared then, either that the coincidences of the chromospheric and coronal lines about 423 and 531 were accidental, or that they were not real coincidences at all. A careful examination of the eclipse photographs of 1896, taken by Mr. Shackleton, and those of 1898, taken by Mr. Fowler, has therefore been undertaken, with special reference to this point.

The wave-length of the coronal ring at 4231, already published in the case of the 1893 photographs, has been confirmed.

The 1896 and 1898 photographs further indicate that the corona line near 4231 is not coincident with the chromospheric line to which reference has been made, and show that while the chromospheric line is coincident with the enhanced line of iron at λ 4233·3, the corona line has a wave-length of 4231·3.

With regard to the ring in the green, the lack of sufficient photographs on isochromatic plates in 1893 does not permit of a final determination of wave-length. Important data, however, were obtained, both in 1896 and 1898. A measurement of the position of the chief ring in the green, as shown in these photographs, comparing the ring with the spectrum of the chromosphere and a solar and iron spectrum taken by the same prisms, shows beyond all question that the wave-length is very different from that generally accepted. The mean result of measurements of different parts of the ring made by Messrs. Fowler and Shackleton and Dr. Lockyer is 5303·7, or about 13 tenth-metres more refrangible than 1474K (5316·79).

Although the new wave-length is not to be regarded as final, for the reason that the conditions under which the photographs were taken necessitate certain small corrections which have not yet been fully worked out, it is not likely that it can be in error by so much as 1 tenth-metre.

The examination of the photographs, which has been undertaken in the first instance by Mr. Fowler, indicates that other important conclusions are to be drawn from the admirable series obtained by him, among them the possible existence of one or more new gases, some of the lines of which, as gathered from the dispersions as yet available, appear also in the spectra of some stars and planetary nebulæ.

The photograph which accompanies this paper has been prepared by Mr. Fowler.

DESCRIPTION OF PLATE 4.

1. Spectrum of Corona and upper Chromosphere.
2. Spectrum of lower Chromosphere, showing that the chromospheric line at 1474K is not coincident with the corona line.
3. *Solar Spectrum.*

1. 2. 3.

December 15, 1898.

The LORD LISTER, F.R.C.S., D.C.L., in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. the Lord Curzon of Kedleston, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Application of Liquid Hydrogen to the Production of High Vacua, and their Spectroscopic Examination." By JAMES DEWAR, LL.D., F.R.S.

II. "On the Boiling Point of Liquid Hydrogen under reduced Pressure." By JAMES DEWAR, LL.D., F.R.S.

III. "Ionic Velocities." By ORME MASSON. Communicated by Professor RAMSAY, F.R.S.

IV. "Note on the Densities of 'Atmospheric Nitrogen,' Pure Nitrogen, and Argon." By WILLIAM RAMSAY, F.R.S.

V. "The Preparation and some of the Properties of Pure Argon." By WILLIAM RAMSAY, F.R.S., and Dr. MORRIS W. TRAVERS.

VI. "Observations on the Anatomy, Physiology, and Degenerations of the Nervous System of the Bird." By R. BOYCE and W. B. WARRINGTON. Communicated by Professor SHERRINGTON, F.R.S.

VII. "The Action of Magnetised Electrodes upon Electrical Discharge Phenomena in Rarefied Gases. Preliminary Note." By C. E. S. PHILLIPS. Communicated by Sir WILLIAM CROOKES, F.R.S.

VIII. "On the Reciprocal Innervation of Antagonistic Muscles. Fifth Note." By C. S. SHERRINGTON, M.A., M.D., F.R.S.

The Society adjourned over the Christmas Recess to Thursday, January 19, 1899.

"The Action of Magnetised Electrodes upon Electrical Discharge Phenomena in Rarefied Gases. Preliminary Note." By C. E. S. PHILLIPS. Communicated by Sir WILLIAM CROOKES. Received November 30,—Read December 15, 1898.

The experiments herein described were undertaken in order to ascertain what would be the action of strongly magnetised electrodes upon electrical discharge phenomena in rarefied gases, and especially upon the charged residual gas, when all external stimulation had ceased.

For this purpose an apparatus was constructed, as shown in Fig. 1, consisting of a soda-glass bulb, B, open at both ends for the purpose of inserting the pointed soft iron electrodes, E_1 and E_2, and with the leading tube L attached for connection to a Sprengel air pump.

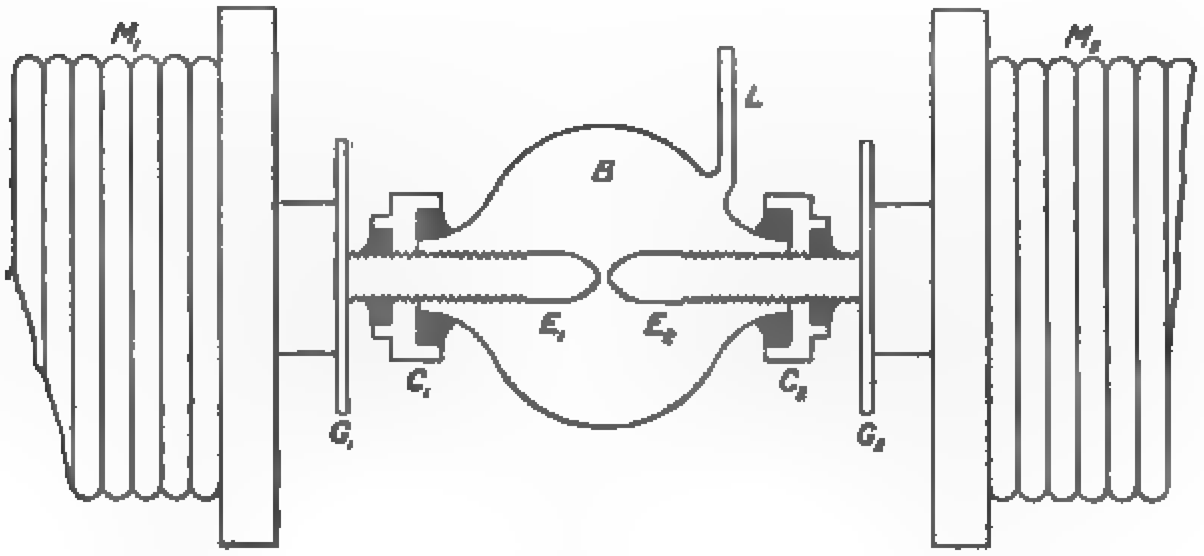


FIG. 1.

No precaution was taken to keep mercury vapour out of the bulb B during the experiments. Each electrode had a screw thread of suitable pitch cut upon it, in order that the brass cups C_1 and C_2, when screwed into position and sealed with cement to the glass, might serve to keep the electrodes central, to reduce the possibility of their rushing together under the influence of strong magnetic forces, and to seal air-tight the two ends of the bulb.

The poles of a large electro-magnet, M_1 M_2, were insulated from the electrodes by means of two thin glass sheets, G_1 G_2. A discharge from the secondary of an induction coil or other suitable source, could then be passed through the bulb, the exhaustion varied and the electrodes magnetised at pleasure.

Results.

A pressure was obtained within the bulb, such that cathode rays *began to* be freely emitted by the negative electrode, and from the

screw thread cut upon it there came a helix of rays which gave rise to the appearance of golden-green rings of fluorescence upon the inner surface of the glass vessel B. When the electrodes were magnetised these green rings twisted somewhat and moved forward, the direction of rotation depending upon the polarity of the magnet emitting the rays.

It has already been noticed* that an external magnet, placed behind the negative electrode will cause rotation of the green fluorescence upon the walls of an exhausted tube, and that the direction of motion is opposite to what it would be in the case of a wire placed at right angles to the axis of the magnet, and in which a current flowed away from the pole. In the above experiment, the rotation was only partial but agreed in direction with the results of other observers.

The bending forward of the rays is evidently dependent upon their partial rotation, because, on reversing the polarity of the magnet emitting them, the direction of their rotation was also reversed, and the bending forward still occurred. That a positively charged body, moving in a definite direction, sets up magnetic whirls in planes transverse to its path, is generally accepted, the whole effect indeed being treated as a current flowing in the same direction; and it seems only logical to conclude that were the body negatively charged, its motion would give rise to effects similar to those accompanying a flow of current in the opposite direction. From this we should expect cathode rays to behave towards a magnet just as would a wire carrying a current in the opposite direction to that in which the charged particles, constituting the rays, are supposed to be moving—a view which is borne out by experiment.

The pressure was then still further lowered, until a 3-inch spark from a 10-inch Apps induction coil was only just able to start the glow. Under these conditions irregular green patches flickered upon the inner surface of the glass; but when the electrodes were oppositely magnetised these green flecks immediately vanished, and, the resistance of the residual gas within the bulb becoming smaller, a hazy blue cloud formed between the points.

Owing to the ever varying charges upon the inner surface of the bulb and upon the electrodes themselves, it could not be ascertained whether this blue cloud tended to assume a definite geometrical form or not. It was found, however, that, after a strong stimulation of the bulb had taken place and then been stopped, the electrodes meanwhile remaining unmagnetised, on exciting the magnet, a luminous ring suddenly appeared within the bulb, between the pointed ends of the electrodes, and in a plane at right angles to the direction of the magnetic lines of force.

The ring shone brightly for a moment, when the magnet circuit was

* 'Phil. Trans,' 1879, Part II, p. 657.

"made," and it was more sharply defined at high exhaustions: becoming in fact hazy and indefinite, if the pressure within the bulb was slightly increased.

On the other hand the rarefaction must not be carried too far, for it is necessary, in order to obtain this luminous effect, that the residual gas within the bulb should be very generally stimulated by the passage of the discharge. The following combinations were then tried:—

After stimulation, both the leading wires attached to the electrodes were removed from the secondary of the induction coil, and (*a*) insulated, (*b*) joined together and insulated, (*c*) joined together and connected to earth, (*d*) one insulated and the other connected to earth. In all these cases the ring formed equally well when the pointed ends of the electrodes were oppositely magnetised. On the other hand, as long as the points were made either both N or both S, no ring could be obtained.

In another experiment, after the exhaustion had been carried somewhat further and the bulb strongly stimulated, a second ring flashed out momentarily when the magnet circuit was completed; it formed concentrically with the smaller and more permanent ring, and appeared to be situated upon the inner surface of the glass bulb.

Observations as to the actual mode of formation of what I venture to call the primary ring, *i.e.*, the smaller one of the two, could at this stage of exhaustion be conveniently made. It appeared to emanate originally as a bright stream from between the electrodes, and then to curl rapidly round the magnetic axis, that portion most distant from the electrodes gaining upon the rest, ultimately disengaging the tail of the stream from between the points, and thus forming an equatorial circle of light within the bulb. The ring then spread out and became somewhat wider and less well defined, and as it gradually died away the glow seemed to be rotating more and more slowly until at last it flickered and vanished.

It appears, in fact, that this luminous ring spins between the electrodes from the moment it forms under the action of the magnet, the high initial velocity with which, in that case, it must be set in motion tending to keep it rotating, even after the magnetic lines have reached a maximum. The gradual expansion of the ring, which begins to take place immediately it has formed, may, according to this view, be due partly to centrifugal force, and also partly to the attraction exerted by an electrostatic charge residing upon the inner surface of the glass walls of the bulb. It is significant, too, that when the ring had all but disappeared, the sudden turning off of the magnet slightly revived the luminosity. At the instant the ring formed, the glass walls of the bulb became charged so strongly that a spark could, in some cases, be seen to pass between the outer surface of the glass and the brass cups, C_1, C_2, attached respectively to either electrode. It should be noticed that the ring is

generally very sensitive to variations in the charge upon the glass walls, and that touching the bulb at various places with the fingers produces vigorous movements of the glow within.

In a bulb, the diameter of which was about 3 inches, the ring threaded itself on to either of the electrodes E_1 or E_2, when the centre of the bulb was electrically connected to the cups C_2 and C_1 respectively.

When only one electrode was magnetised, after the bulb had been stimulated, the rotation of the glow was more easily seen owing to the formation around the magnetised electrode of a wide, spiral-shaped, luminous cloud which was apparently rotating as it became more and more dim, and it was then noticed that the direction of rotation could be reversed by reversing the polarity of the magnetised electrode. No change in the effect was observed when the connections to the bulb were reversed.

The form of the electrodes was next varied, but the effects produced were mainly the same. With a pointed cathode and a concave anode the ring formed as usual, but it was observed that, whenever these relations were reversed, no ring could be obtained. Indeed, having first of all stimulated the bulb with the concave electrode negative, it was not only impossible to obtain a ring on magnetising the electrodes, but even when the connections were reversed still no ring would form until after repeated or prolonged stimulation of the bulb. Neither did a ring form when the electrodes were magnetised so that like poles faced each other—a similar result to that already observed with pointed electrodes.

It may be worth recording that when pointed electrodes were employed, the ring formed equally well, whether the bulb was stimulated by means of a Tesla oscillator, an induction coil, or a Wimshurst influence machine.

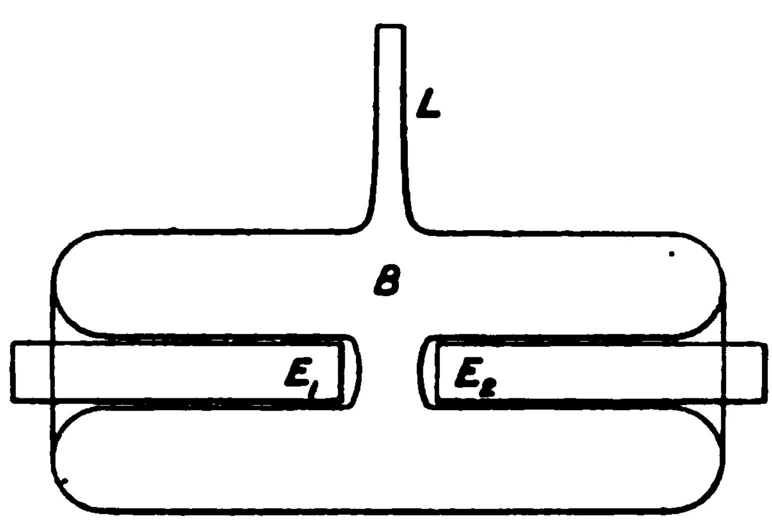


FIG. 2.

Finally, experiments were made with external magnetised electrodes, the exhausted bulb being shaped as shown in fig. 2. In this case of course, the discharge was oscillatory, and consequently the effects were not very directly comparable to those already described.

At a low pressure, however, and when the bulb was filled with a

green glow, on disconnecting the induction coil and magnetising the electrodes, a flash of white light was observed within the tube, and irregular green splashes momentarily made their appearance upon the glass.

In conclusion, I desire to offer my thanks to Dr. Silvanus P. Thompson, F.R.S., for kindly permitting me to use the Tesla apparatus at his laboratory, and for the interest he has taken in the progress of these experiments.

"Observations on the Anatomy, Physiology, and Degenerations of the Nervous System of the Bird." By R. Boyce and W. B. Warrington. Communicated by Professor Sherrington, F.R.S. Received December 7,—Read December 15, 1898.

(Abstract.)

In this research the modern methods of investigating the course of tracts and their degeneration in the central nervous system have been used. The previous literature of the subject is scanty. Bumm first gave an account of the various tracts in the brain, and the histological side has been and is still being worked out by Brandis.

Valuable information is given by Edinger in his 'Vorlesungen,' and quite recently the Marchi method has been used and the results obtained described by Wiener and Münzer, Wallenberg and Friedländer.

The anatomy has been studied by sections made in the three planes and stained by the Weigert and Nissl methods, and by observing the course of the degenerated fibres following various lesions, staining by Marchi's fluid.

In the brain of the Bird the cortex of higher animals is represented by a thin pallial sheet of grey matter, forming the mesial and dorsal boundary of the narrow ventricle, and gradually losing itself on the lateral aspect of the hemisphere. Its substance is composed of oval, rather large cells, grouped into clusters, and it contains the fibres of an important tract, called by Edinger the Tr. septomesencephalicus, and by us alluded to as the pallial tract. The hemispheres themselves correspond to basal ganglia; posteriorly they expand laterally into the large occipital lobes. Their substance contains cells resembling those found in the pallium, and which cannot be differentiated distinctly into definite regions.

The hemispheres are connected with the thalamus by a constriction of their substance, forming an isthmus on either side, from which the thalamus in transverse section is seen suspended as a triangular shaped *body.*

The other prominent features are the large optic lobes, on the surface of which shining white fibres can be seen, and also the excessive size o the optic tracts, the diameter of which is equal to that of the spina cord in the dorsal region.

The general plan of the tracts of the central nervous system is as follows:—

I. From the hemispheres tracts arise which undergo a descending degeneration, and terminate in the thalamic and mesencephalic region There is no direct connection of the cerebral hemispheres with the spinal cord.

II. A well marked tract arises in the mesencephalon, undergoes an ascending degeneration, and can be found to terminate in the substance of the hemisphere.

III. The mesencephalon is the site of a complex system of fibres which can be grouped as follows:—

(α) The ascending tract to the hemispheres alluded to above (Tr mesencephalicus striatus).

(β) Arciform fibres decussating in the middle line and reaching downwards into the spinal cord (Forel's and Meynert's foun tain decussation).

(γ) The optic tracts and various commissural fibres connected with this.

(δ) A tract originally described by Perlia, and called by him th median optic bundle.

This tract can be well seen after all lesions involving the optic lobe as a well defined degenerate bundle, situated on the inner side of th dorsal aspect of the optic tract, and ending in the ganglion isthmi which is situated in the optic lobe at the junction of that body with the cerebellum and pons. Peripherally, Wallenberg found that i could be traced to the ganglion layer of the retina of the opposit eye, and Perlia, in his original description, observed the tract as degenerated bundle found after enucleation of an eye.

IV. The commissural system, including—

(α) The anterior commissure.

(β) The posterior commissure.

(γ) The commissure of the roof of the aqueduct (lamina commis suralis mesencephali) which contains the large cells though to be the mesencephalic nucleus of the trigeminus.

(δ) The small pallial commissure. Hippocampal commissure c Elliot Smith; commissura anterior et posterior pallii c Edinger.

V. The tracts of the spinal cord, which have broadly the usua ascending and descending course.

Without entering into detail, some points in connection with the tracts of the cerebral hemisphere should be mentioned.

(i) The Tr. septomesencephalicus forms a prominent feature on the mesial wall of the hemisphere, where it is seen as a fan-shaped expansion of white fibres. It rapidly converges to a well defined bundle, which turns laterally, and is seen on the ventral aspect of the brain as a band of fibres situated between the optic lobes and hemispheres. Sections show that the anterior lobe of the brain contributes to its formation, and that it terminates in the epithalamic region. Whilst the main mass of its fibres pass in front of the anterior commissure, a distinct band passes posteriorly to that commissure to end in the region of the ganglion habendulae. This part represents the fornix. It also gives origin to fibres of the "pallial" commissure, and is in connection with the optic tract by a well defined bundle.

(ii) The middle region of the hemisphere especially, but also the remaining parts to a less extent, give origin to the tracts which terminate in the thalamus and mesencephalon respectively, viz., the Tr. striothalamicus and Tr. striomesencephalicus.

(iii) The expanded posterior part of the hemisphere is the site and origin of three large tracts, viz., the anterior commissure, the Tr. occipitomesencephalicus, and a great associational bundle binding the anterior and posterior parts of the brain together, and called the fronto-occipital tract.

In considering the physiological significance of these tracts, their anatomical distribution indicates the paramount importance of the sense of sight in the bird.

The optic tracts and lobes are enormously developed, and the latter have connections with all parts of the central nervous system, being thus a reflex centre of the highest importance. The well-developed posterior parts of the hemisphere, with their connections with the mesencephalon, illustrate in this animal the first formation of a higher cerebral visual centre.

Further, not only is there marked deficiency of sight in the opposite eye after injury to one optic vesicle, but the same symptom is noticed after injury to any part of the cerebral hemisphere. The defect of sight is most marked after removal of the whole hemisphere, or of the occipital portion; but we are of opinion that distinct amblyopia follows a superficial lesion chiefly involving the pallial tract, or after removal of the fore part of the brain, which, as mentioned above, is connected with both this tract and with the occipital lobe.

Two excitable areas are found on the surface of the hemisphere:

One situated on fibres of the pallial tract, near the median plane, and stimulation of which gives, as was long ago described by Ferrier, constant contraction of the pupil of the opposite eye.

By what path such stimulation affects the motor nuclei we do not know, but lesions of this region give rise to a limited degeneration in the pallial tract.

A second situated on the lateral aspect of the surface of the brain, at a point corresponding to the junction of the great striate and occipital tracts.

Stimulation of this area gives rise to complicated movements, which consist chiefly of deglutition, often accompanied by actual pecking and by a rotation of head and neck.

No other motor symptoms were noticed on carefully stimulating the surface of the brain. Nor is any motor defect observed after removal of one hemisphere. After removal of both hemispheres, a condition follows which has been carefully studied by Schrader, and with the description of this author we fully agree. The symptoms vary according to the time which has elapsed since the operation. In the early stage the animal is markedly inert, stands with flexed head, ruffled feathers, and eyes shut; the lack of initiative is pronounced. In the later stage it constantly walks about, and is in a condition of continual unrest, yet always avoids obstacles, and can maintain its equilibrium in various positions.

The relative importance of the mesencephalic spinal system of fibres led us to examine the animals after injury of the optic vesicles for indications of motor defect.

Contrary to what has been noticed in higher animals, we are of opinion that whilst a slight lesion is not followed by any observable motor defect, more pronounced injury gives rise to a weakness on the opposite side, so that the animal falls to that side. If the lesion be very severe, the animal is quite unable to stand, and lies continually on its back.

"On the Reciprocal Innervation of Antagonistic Muscles. Fifth Note." By C. S. Sherrington, M.A., M.D., F.R.S. Received November 29,—Read December 15, 1898.

In a previous communication upon this subject, I gave* the results obtained in an experimental examination of the antagonistic correlation which at least potentially exists in the muscular action of the opening of the palpebral aperture. The *orbicularis palpebrarum* and the *levator palpebræ superioris* are to a certain extent an antagonistic couple. During the course of last year I took opportunity to examine the co-ordination of the same antagonistic muscles in the movement, not of

* 'Journal of Physiology,' vol. 17, p. 27, 1894.

the opening of the palpebral fissure, but of its closure. The observations having been unavoidably interrupted by removal to a new laboratory, it is only recently I have been able to confirm the preliminary observations on a sufficiently extended scale.

The monkey and the cat have been the animals employed. Under deep chloroform narcosis intracranial section of the VIIth cranial nerve was performed at the point where the nerve plunges into the petrosal portion of the temporal bone. In three instances the *nervus octavus*, and the *pars intermedia*, were also severed with the *facialis*. In every case the side selected for operation was the left. The facial palsy caused was not detectable so long as the narcosis was maintained. As that was gradually recovered from, asymmetry of expression, &c., became marked. In those instances in which both the facialis and octavus had been severed, there appeared among the symptoms the following:—Rotatory nystagmus of the left eyeball, some inequality of the pupils—the left being the smaller, some degree of impotence of the eyeballs to move so far to the right as to the left, or, expressed more objectively, the eyeballs were never observed to move freely to the right of the primary visual position, although they frequently moved well to the left; they certainly never moved far to the right; the animals rolled over about the long axis of the body, as mentioned in Magendie's original description of the effect of unilateral section of the pons. The direction of rotation, if traced from the supine position as starting point, was towards the animal's right side, so that that side next after the back lay undermost. The monkey clutched hold of things within reach, with the apparent intention of preventing itself from rolling. If it failed to obtain some support the rolling would continue through a series of complete turns. This was the condition immediately after complete recovery from the narcosis, and at that time the left knee-jerk was less brisk than the right; on the latter side it appeared to be abnormally brisk, but it is difficult to fix a normal. The actual existence of section, and whether it had included both nerves or only one, was always determined by subsequent post-mortem dissection.

As to the eye closure, while the animal was exhausted, or sleepy, or only partially recovered from the chloroform narcosis, there was no obvious difference between the appearance of the eyelids on the two sides, as they rested half open over the globes. When the animal blinked, however, under these conditions, the palpebral opening of the right eye closed, but not that of the left—at least not to any easily perceptible extent. When on the contrary the animal was fully awake and active, with both eyes well opened, it was seen that as the right eye blinked the left eye also did so. By blinking I understand the rapidly executed movement of closure which occurs so repeatedly without *attention* being directed to it, although it can be voluntary restrained—

the quick movement which may be regarded as an irregularly recurring reflex that doubtless has among its objects the renewal of moisture on the corneal surface, which otherwise would become dry. This natural blinking movement seems in the monkey not to employ the orbital portion of the *orbicularis palpebrarum*, but only the palpebral. It occurs habitually as a bilateral and symmetrical movement. It is far less extensive in action than the closure of the palpebral opening, which ensues when the monkey grimaces on being threatened with a blow. That in the blinking the contraction of the palpebral part of the orbicularis is not however the whole of the muscular mechanism at play, is clear from the fact, that in the awake and active animal with fully opened eyes, the blinking still remains bilateral, subsequent to section of the facialis nerves of one side. The blinking by the right eye was of course normal in character. As the right eye blinked, the upper lid of the left eye quickly dropped three to four millimetres over the globus of that side, and was then synchronously with the lifting of the right upper lid lifted again. The left lower lid was not on any occasion detected to move at all. The quick fall of the upper lid of the left eye must have been due under these circumstances to inhibition of the tonus of the left levator palpebræ superioris muscle. This brings the co-ordination of the reaction into line with that which I have described for other movements under the term reciprocal innervation.

It is interesting that Panas, Sappey, Fuchs, Wilmart and others, who have carefully and particularly studied the mechanism of the closure of the eye, have not attributed any share to an inhibition of the levator palpebræ; one physician, however, Dr. Lor, of Brussels, has argued that in the closure of the human eye such an inhibition does under certain circumstances occur.

"Note on the Densities of 'Atmospheric Nitrogen,' Pure Nitrogen, and Argon." By WILLIAM RAMSAY, F.R.S. Received December 3,—Read December 15, 1898.

M. A. Leduc in a recent paper* has discussed the relation between the density of argon, its proportion in atmospheric nitrogen, the density of the latter, and that of pure nitrogen. It appears to me that he has misunderstood some of the data given by Lord Rayleigh, Dr. Kellas, and myself; and as the question whether the found density of argon corresponds with that calculable from the other data, is in itself an interesting one, I have the honour to present this note to the Society.

* "Recherches sur les Gaz," 'Ann. Chim. Phys.,' September, 1898.

The data may be divided into two groups: those of Leduc and Schloesing; and those of Rayleigh, Ramsay, and Kellas.

From the first group it is possible to calculate the density of argon, *i.e.*, the crude mixture left after separating oxygen and nitrogen from air.

From the second group, the density of argon may be calculated; or if that be assumed, both groups give data for the calculation of that of "atmospheric" nitrogen. It has been thought better to express the results in the form of the weight of one litre of the gas in question; but if it is desired to state them with reference to the density of oxygen = 16, the conversion may be made by means of the weight of a litre of oxygen according to both Lord Rayleigh and M. Leduc.

The data are as follows:—

Weight of 1 litre of	Leduc.	Rayleigh.	Schloesing.	Kellas.	Ramsay.
Air..................	1·29316	1·29327			
Oxygen............	1·42920	1·42952			
Nitrogen	1·25070	1·25092			
„ (atmo.)	1·25700	1·25718			
Argon	...	1·78151	...	...	1·7816
„ in "atmo." nitrogen......	...	...	0·01183	0·01186	

Weight of 1 litre argon calculated from Leduc's and Schloesing's figures:—

$$0{\cdot}01183x = 1{\cdot}25700 - (1{\cdot}25070 \times 0{\cdot}98817); \text{ hence } x = 1{\cdot}7828$$

The difference from the value found is 7 in 10,000.

Weight of 1 litre argon calculated from Rayleigh's and Kellas's figures:—

$$0{\cdot}01186x = 1{\cdot}25718 - (1{\cdot}25092 \times 0{\cdot}98814); \text{ hence } x = 1{\cdot}7791.$$

The difference from the value found is 13 in 10,000.

Both of these results are quite satisfactory, considering that the nature of the calculation involves a ratio of small differences. The agreement is more striking if the density of "atmospheric" nitrogen is calculated from the figures; for this calculation, the weight of 1 litre of argon is assumed to be 1·7815 grams.

Weight of 1 litre of "atmospheric" nitrogen from Leduc's and Schloesing's figures:—

$$x = (1{\cdot}7815 \times 0{\cdot}01183) + (1{\cdot}25070 \times 0{\cdot}98817); \text{ whence } x = 1{\cdot}25698.$$

Here the difference is only 2 in 125,000.

From Lord Rayleigh's and Dr. Kellas's figures, we have:—

$$x = (1{\cdot}7815 \times 0{\cdot}01186) + (1{\cdot}25092 \times 0{\cdot}98814); \text{ whence } x = 1{\cdot}25721.$$

The difference here is only 3 in 125,000.

It is thus evident that either set of figures gives results as concordant as could be wished; and that the density of "atmospheric" nitrogen is correctly given as the mean of the densities of the constituents, taken in the proportion in which they occur.

"The Preparation and some of the Properties of Pure Argon." By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS. Received December 12,—Read December 15, 1898.

In the memoir on Argon, a new constituent of the atmosphere,* by Lord Rayleigh and one of us, reasons are adduced on pages 235 and 236 in favour of the supposition that argon is an element, or a mixture of elements; and on page 236, the following words occur:—"There is evidence both for and against the hypothesis that argon is a mixture, for, owing to Mr. Crookes's observations of the dual character of its spectrum; against, because of Professor Olszewski's statement that it has a definite melting point, a definite boiling point, and a definite critical temperature and pressure; and because on compressing the gas in presence of its liquid, pressure remains sensibly constant until all gas has condensed to liquid. The latter experiments are the well known criteria of a pure substance; the former is not known with certainty to be characteristic of a mixture." And on pages 257–259 of the same volume, it is shown by Professor Sydney Young and one of us, that the ratios between the boiling points of argon and benzene, argon and alcohol, and argon and oxygen on the absolute scale are such that it is possible to compute the boiling points of argon at different pressures with very considerable accuracy. We therefore draw the conclusion:—"It is hardly likely, though not impossible, that so good an agreement would be obtained with a mixture or an impure substance. It is, at any rate, certain that a distinct want of agreement would have shown that argon was not a definite, pure substance, and the results may be taken as affording additional confirmation of the conclusion that argon is a definite, hitherto unknown constituent of the atmosphere, and that it has been isolated in a state very closely approaching to purity."

The density of a sample of argon prepared by means of magnesium was found by one of us to be 19·941 (O = 16); and a much larger preparation by Lord Rayleigh, obtained by exposing a mixture of air and oxygen to an electric flame in presence of caustic soda, possessed the density 19·94. Supposing argon to be a simple substance, and not a mixture, the atomic weight would therefore be 39·88. An attempt was made by Dr. J. Norman Collie and one of us to effect a separation

* 'Phil. Trans.,' A, (1895), p. 187.

of argon into a light and a heavy portion passing by diffusion; but without definite results. While the density of the portion passing first through the pipe-clay septum was 19·93, that of the last portions was 20·01. We remarked on these figures in the following terms:—"These numbers show that no important separation has been effected. The difference in density of the two portions may possibly be attributed to experimental error. . . . As it stands, the difference is an extremely minute one, and it may, we think, be taken that any separation of argon, if effected at all, is very imperfect."

It thus remained uncertain whether argon was a mixture or not; although the balance of evidence went against the supposition.

When helium was discovered in 1895, a new light was thrown on the question. Its density, determined by Ramsay, and independently by Langlet, closely approximates to 2·0. Experiments, in conjunction with Dr. Collie, and subsequently by ourselves, showed that no great difference in density could be brought about by fractional diffusion; and, indeed, the latter revealed no impurity except traces of argon. On grounds similar to those from which a corresponding conclusion for argon was drawn, the atomic weight of helium must be somewhat below 4·0. The difference between the atomic weights of helium and argon is consequently about 36; and this is approximately the difference between the atomic weights of manganese and fluorine (36), chromium and oxygen (36·3), vanadium and nitrogen (37·4), and titanium and carbon (36·4). But between each of these pairs of elements, there exists another, exceeding in atomic weight the lower member of each pair by approximately 16. It was therefore to be expected that another element should exist, with atomic weight about 20, and on the assumption that, like helium and argon, it too would be a monatomic gas, its density would be 10. It was with the object of attempting to discover this unknown gas that the experiments on the fractional diffusion of helium were made; and the gases evolved on heating some fifty minerals were investigated in order to find out whether a new spectrum could be observed, but with negative results. Seven meteorites were also examined, as well as six mineral waters, but no new lines could be found. Sixteen of the minerals, two of the mineral waters, and one of the meteorites were proved to contain helium; but if the gases extracted from them contained any gas other than helium or a trace of argon, it must have been in quantity too minute to have revealed itself to the spectroscope.

The gas which we were in search of was ultimately found in argon. The argon, amounting to about 15 litres, was prepared by means of apparatus, of which a description is given in the sequel. From the air employed in a liquid state for the purpose of liquefying and fractionally distilling the argon, two other elementary gases have been obtained, besides one yielding the "Swan" spectrum. A preliminary

account of all these gases has already been given in the 'Proceedings' under the names of "Neon," or "new," "Krypton," or "hidden," and "Metargon"; and at the meeting of the British Association at Bristol, the discovery of "Xenon" or "the stranger" was announced. The removal of these gases from argon has put us in possession of pure argon, and the present paper deals with some of its properties.

The Properties of Pure Argon.

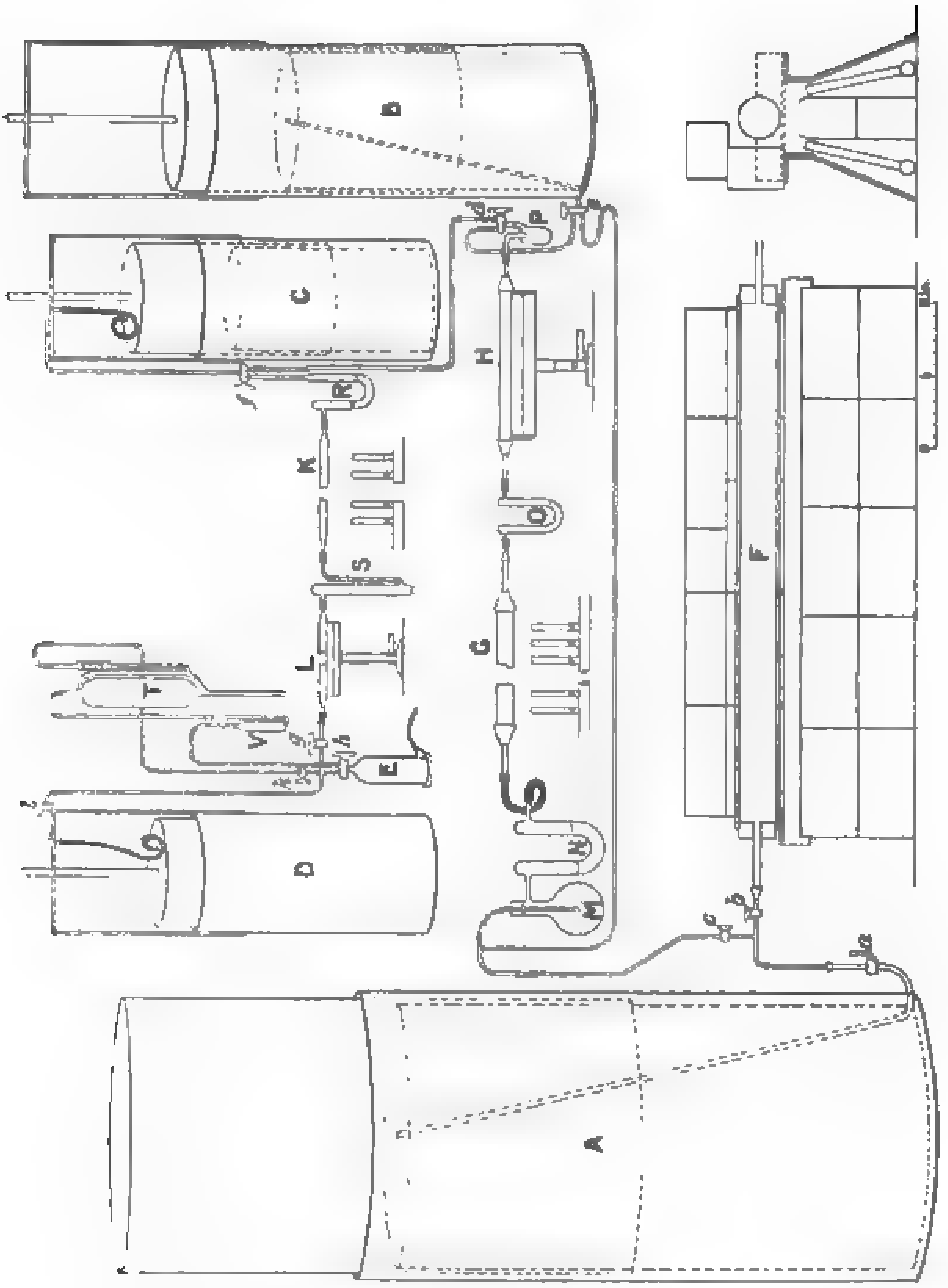
In order to prepare 15 litres of argon, it is necessary to deal with about 1,500 litres of atmospheric air, of which approximately 1,200 litres consist of a mixture of nitrogen and argon. To absorb the nitrogen contained in this quantity of gas by conversion into nitride, 4 kilograms of magnesium would be required theoretically, but in order to cover loss through leakage and incomplete action, 5 kilograms of the metal are employed. The absorption of the oxygen and nitrogen was conducted in three stages. In the first, the oxygen was removed by means of metallic copper; in the second, the nitrogen was passed twice over metallic magnesium; and in the third, the gas, now rich in argon, was finally freed from nitrogen and hydrogen by passage over a mixture of anhydrous lime and magnesium powder heated to a red heat, and subsequently over red-hot copper oxide. The apparatus employed is shown in detail in the annexed figure.

It was of course necessary to confine the gas over water between the successive stages of purification, and finally to store the gaseous argon in the same way. On account of the considerable solubility of argon in water, this would have entailed no small loss if the quantity of water with which it had been brought into contact had been large. We consequently decided to make use of gas-holders of the gasometer type, in which the water was contained in an annular space of small capacity. Balance weights were attached to cords passing over pulleys, and served to relieve the pressure on the gas due to the weight of the gasometer. As the volume of the gas decreased after each successive stage, the four gas-holders employed were of different sizes; the capacity of A was about 180 litres; that of B, 27 litres; and of C and D, each 18 litres.

Atmospheric nitrogen was obtained by drawing air, freed from carbon dioxide by passage through caustic soda solution, over heated metallic copper. A large iron tube F, 3 feet 6 inches long, and 3·5 inches in diameter, containing 25 lbs. of scrap copper, was connected with the gas-holder A; the tube was heated in a long fire-brick trough during these experiments, but a gas-furnace is shown in the figure, which has now been substituted for the more primitive arrangement.

The time required to fill the gas-holder was usually about five hours, and it was found, on analysis of the gas, that one single operation

Fig. 1.



sufficed for the complete removal of all oxygen. The oxidised copper was reduced between each operation by means of coal-gas.

By closing the stop-cock *b*, and opening the stop-cock *c*, the gas-holder A could be placed in communication with the apparatus in which the preliminary absorption of nitrogen took place. By placing weights on the top of the gas-holder, the nitrogen was driven through the vessel M, and the U-tube N, both of which contained strong sulphuric acid, into the tube G, which contained magnesium. This *tube was* a piece of steam-barrel, 1·5 inches in diameter, connected at

each end by a reducing socket with an iron tube, 0·25 inch in diameter. The tube contained 250 grams of magnesium, cut into coarse shavings in a shaping machine; the magnesium was not pressed very tightly into the tube. Since after each operation it was necessary to remove the sockets in order to clear the tube, the joints were luted with red lead, and the tube was made of sufficient length to project about 3 inches at each end of the furnace.

The greater part of the nitride was generally removed by using an iron rod, and the remainder by means of water, which converted it into the hydroxide. The tube was raised to a bright red heat before connecting it with the U-tube O, in order to allow the greater part of the hydrogen occluded by the magnesium to escape. The absorption of the nitrogen, which was indicated by the rate of flow of the gas through the U-tubes N and O, was maintained briskly until practically the whole of the magnesium was converted into nitride; the volume of the gas absorbed was equivalent to half the capacity of the large gasholder.

The gas, after leaving the U-tube O, passed through a second iron tube H, containing copper oxide; next, through the vessel P, in which water condensed; and it finally collected in the gasometer B. That which passed during the first stages of the process consisted of nitrogen containing much argon; but towards the end of the operation the argon became much diluted, until finally the gas which passed through the U-tube O consisted almost entirely of atmospheric nitrogen. The tube G was then replaced by another containing a fresh supply of magnesium.

The tap *c* was then closed, and the taps *d* and *e* turned, so that the gas in the gasometer B could be made to flow through the magnesium and copper oxide tubes into the gasometer C. In this process its volume was very much reduced, and the gas which collected in C probably contained as much as 25 per cent. of argon. When the whole of the gas had been expelled from B, the taps *d* and *e* were again turned, and atmospheric nitrogen was allowed to flow through the magnesium tube, as in the first stage of this operation.

When the gasometer C had become full of the mixture of nitrogen and argon, as it did at the end of every third or fourth operation, it became necessary to reduce its volume by further absorption of nitrogen. The method employed, which was first described by Maquenne,* consisted in passing the gas through a hard glass tube containing a mixture of magnesium powder and lime, heated to a dull red heat in a combustion furnace. The lime was obtained by thoroughly calcining precipitated chalk in a muffle. The nitrogen continued to be completely absorbed as long as calcium remained unattacked, so that the product of this operation consisted of pure

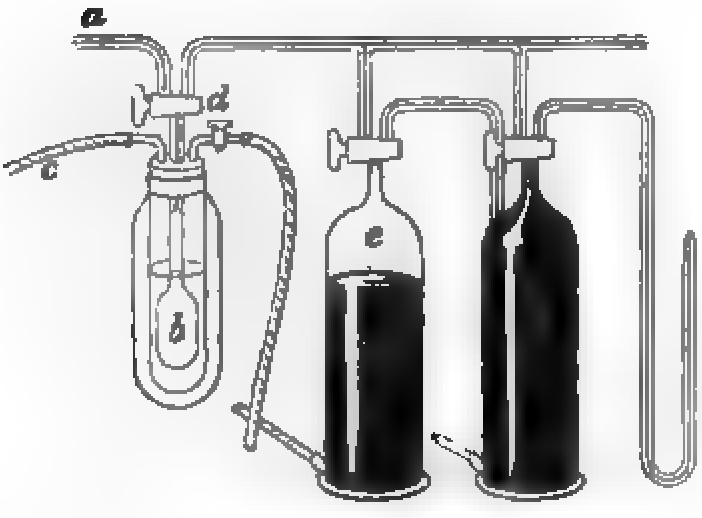
* 'Compt. Rend.,' 1895, vol. 121, p. 1147.

argon. The gas issuing from the calcium tube passed through a tube S, containing soda-lime, and over copper oxide in the tube L, on its way to the gasometer D. Since at the end of the operation the system of tubes between the gasholders C and D contained argon, in order to avoid loss, the circuit was placed in communication with a Töpler pump T, through the stopcock *k*. The space between the stopcocks *l* and *f* was exhausted at the commencement of the operation, the exhaustion being continued till the greater part of the hydrogen, which is always evolved when a mixture of magnesium and lime is heated, had been given off.

When it was necessary to suspend operations, the taps *l* and *f* were closed, the tap *k* was opened, and the argon was taken into the pump and delivered into the vessel V which covered the upturned end of the capillary tube of the pump. From V the argon could be drawn into the small gasholder E, which contained mercury, and which could also be placed in communication with the system through which the gas passed on its way from C to D.

These operations were repeated until the gasholder D contained about 15 litres of argon. The whole of this argon was then liquefied in an apparatus of which fig. 2 is a representation. The argon

FIG. 2.



entered through the tube *a* into the bulb *b*, of some 25 c.c. capacity, surrounded by liquid air contained in a double-walled vacuum jacket. The air was made to boil under a low pressure of a few centimetres of mercury by means of a Fleuss pump attached to the tube *c*. The argon rapidly and completely liquefied to a colourless mobile liquid; it showed no absorption spectrum. Its volume was about 17·4 c.c. By turning the tap *d* it was placed in communication with the first of the series of mercury gasholders, *e*; the reservoir was then lowered so as to remove the lower-boiling portions of the liquid. During this distillation, which took place at constant temperature, the pressure on *the boiling air* was kept as low as possible. This gas subsequently

turned out to be rich in neon, and to contain helium.* The remainder of the argon boiled back into the gasometer until the last few drops were left; the residue solidified, and finally gave a gas to which we gave the name metargon; it was collected in mercury gasholders.† As will be subsequently shown, the krypton and xenon in this quantity of argon are too minute for detection. A similar operation for the purpose of separating the lighter as well as the heavier constituents was afterwards repeated three times, the middle portion of argon being always returned to the gasholder D (fig. 1). A fourth liquefaction was carried out in which six mercury gasholders were filled with six separate fractions of argon, each taken after each successive fifth of the total argon had evaporated. These fractions were next purified from any nitrogen accidentally present by sparking with oxygen over caustic potash. After the removal of the oxygen the density was determined.

Density of Argon.

For a preliminary determination of the density of the various samples a bulb of about 33 c.c. capacity was employed. It is much easier to ensure the purity of a small sample of gas than of a large one; and it will be seen that very concordant determinations are obtainable with a small quantity. The limit of error is probably not greater than one part in a thousand. The results are expressed in terms of O = ·16.

	Capacity of bulb. c.c.	Temp.	Pressure. mm.	Weight. gram.	Density.
(1)	32·762	19·05°	535·1	0·03786	19·65
(2)	,,	15·70	712·0	0·05265	19·95
(3)	,,	17·00	662·2	0·05012	19·95
(4)	,,	14·55	749·8	0·05460	19·91
(5)	,,	15·60	740·4	0·05389	19·97
(6)	,,	16·15	760·2	0·05501	19·95

The spectrum of No. 4, examined later, showed a trace of nitrogen; the density of No. 6 was confirmed by other two determinations, each made after further sparking.

No. 1 was the first portion boiled off, and therefore its density is lower than that of the other fractions, probably owing to its still containing some neon and helium. The rest of the samples have a constant density, approximately 19·95.

A larger quantity of No. 5 was then purified by long-continued sparking, and its density was determined in a bulb of greater capacity. To show the influence of such purification results are given, obtained

* 'Roy. Soc. Proc.,' vol. 63, p. 437.

† *Loc. cit.*, p. 439.

before it was complete. The gas under such conditions showed a trace of the nitrogen spectrum. The portion last weighed was spectroscopically pure.

Capacity of bulb.	Temp.	Pressure.	Weight.	Density.
c.c.				
163·19	15·47°	767·1	0·27235	19·935
„	16·97	764·8	0·26985	19·914
„	13·34	742·8	0·26591	19·952
„	12·95	741·3	0·26586	19·961

After the first of these determinations the gas was passed over a mixture of red-hot magnesium and lime, and subsequently over red-hot copper oxide, in order to remove hydrogen. But after determining the density, the gas was examined spectroscopically, and was found to contain hydrogen. The gas was therefore again sparked, when the density 19·952 was found. This specimen was also examined spectroscopically, and was found to be absolutely free from all visible traces of impurity. The last weighing refers to the same sample of gas, and was made as a control experiment.

These results conclusively prove that the density of argon, purified from its companions, does not differ greatly from that obtained by Lord Rayleigh, viz., 19·94, nor by one of us, viz., 19·941. The true density may, we think, be safely taken as the mean of the last two determinations, viz., 19·957.

This corresponds with the mean of the four reliable determinations with the small bulb, viz., Nos. 2, 3, 5, and 6, which is 19·955.

Refractivity of Argon.

The refractivity of pure argon was next determined. The measurements were made according to the plan suggested by Lord Rayleigh.* The samples investigated were Nos. 1, 2, 5, and 6. The comparison was made with air.

(1)	0·9620	Contains neon and helium.
(2)	0·9687	
(5)	0·9647	Mean, 0·9665.
(6)	0·9660	

The refractivity of a previous sample of argon, obtained from the middle of the 15 litres, during the second liquefaction, was 0·9679, a number differing only slightly from that given above.

The refractivity of argon containing krypton, which had a density 20·01, was much higher than the number given above for pure argon, for it reached 1·030 as a mean of two determinations. Evidently then

* 'Roy. Soc. Proc.,' vol. 59, p. 201.

the body possessing the high refractivity was not present in No. 6 in greater proportion than in No. 2, otherwise the refractivity of No. 6 would have shown an increase over that of No. 2.

The refractivity of pure argon differs somewhat from the value for crude argon found by Lord Rayleigh, viz., 0·961,* and also from that previously found by ourselves, 0·9596. The removal of neon, which appears to have a very low refractivity, and of helium, of which the refractivity is 0·1238, accounts for the increased refractivity of a sample from which they are absent. The gases which we have recently found in air and in crude argon will form the subject of a future communication. Suffice it to say that the amount of neon and helium is much more considerable than that of the others, and that their effect on crude argon is, therefore, much more marked on its density and refractivity.

To revert to the opening sentences of this communication, we may point out that the remarks on the homogeneous nature of argon were just at the time. The change in its physical constants, caused by the mixture of more recently discovered gases which it has been shown to contain, is exceedingly small, and does not call for any serious alteration in the original paper on "Argon, a new Constituent of the Atmosphere."

The Density of Argon at the Boiling Point of Oxygen.

In an addendum to the original paper on argon,† the expansion of argon by rise of temperature to 250°, as well as its contraction by fall of temperature to −88°, was determined. There is a considerable difference between the temperature at which nitrous oxide boils and that at which oxygen boils, and it was thought worth while to ascertain whether argon behaves as a normal gas down to the boiling point of oxygen. Olszewski‡ gives the boiling point of argon as −187°, and that of oxygen as −182·7°; at the latter temperature, therefore, argon would not be far removed from its own condensing point. The interesting question, of course, is the possible polymerisation of argon at such a low temperature.

No sign of any polymerisation has been observed, as is shown by the following data:—

Hydrogen Thermometer.

Temperature. °C.	Pressure. mm.	Volume.	R.
99·7	1091·5	1·0026	2·9362
0·0	803·2	1·0000	2·9421
−182·7	269·6	0·9953	2·9715

* 'Roy. Soc. Proc.,' vol. 59, p. 205.

† 'Phil. Trans.,' A, 1895, p. 239.

‡ *Loc. cit.*, p. 257.

Argon Thermometer.

Temperature. °C.	Pressure. mm.	Volume.	R.
100·1	1414·9	1·0026	3·8095
0·0	1040·0	1·0000	3·8022
−182·7	353·2	0·9953	3·8930

No correction has been made for the unheated or uncooled stem of the thermometer; but it is obvious that although the lowest temperature lies close to the boiling point of argon, the ratio of the values of PV/T of hydrogen and argon at that temperature, as well as the others, is practically constant.

"On some Expressions for the Radial and Axial Components of the Magnetic Force in the Interior of Solenoids of Circular Cross-section." By C. COLERIDGE FARR, B.Sc., formerly Angas Scholar, University of Adelaide. Communicated by Professor H. LAMB, F.R.S. Received June 7,—Read June 16, 1898.

In the present paper, certain expressions are arrived at, in terms of zonal spherical harmonics and their first derivatives, by which the values of the two components of the magnetic force may be calculated for any point in the interior of a coil, and hence the total force may be found both in magnitude and direction. The resulting series suffer from the well-known defect in the spherical harmonic method, in that they are not very rapidly converging for points near the boundary of the space for which they apply. A table of the values of the first derivatives of the first seven zonal harmonics is added. This table, in conjunction with that calculated by Messrs. Holland, Jones, and Lamb, and published in the 'Philosophical Magazine,' Series 5, December, 1891, will facilitate the numerical use of the expressions arrived at.

Let Ωda be the magnetic potential at any point within a solenoid whose depth of winding da is indefinitely small. If a be the radius of this "solenoidal sheet," and the axis of z be the axis of the coil, the axis of x being along a radius of the circular section of the coil, and the origin at the centre of the equatorial section of the coil, we have, neglecting the insulating covering of the wire,

$$X = -\int_{T}^{S} \frac{d}{dx}(\Omega da), \qquad Z = -\int_{T}^{S} \frac{d}{dy}(\Omega da),$$

where X = the radial component of the magnetic force,

Z = the axial component,

S = radius of the outside winding of the coil of finite dimensions,

T = radius of the inside winding.

Now $$\Omega da = n\gamma da\,[-4\pi z + V_1 - V_2]^* \quad \ldots\ldots\ldots\ldots\ (1),$$

where n = the number of turns per unit length *per unit increase in radius*,

γ = current in C.G.S. units,

V_1 = potential at the point considered due to a plane area of surface density unity, coinciding with the positive end of the coil,

V_2 = potential due to the negative end.

If r and r' be the distance of the point considered from the centres of the positive and negative ends of the coil respectively, and P_n and Q_n the nth zonal harmonics corresponding to the angles θ and ϕ which r and r' make respectively with the axis of the coil, we have

$$V_1 = 2\pi\left(-rP_1 + a + \Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!}{(2p-1)\,2^{2p}\,(p!)^2}\frac{r^{2p}P_{2p}}{a^{2p-1}}\right),\dagger$$

when $r < a$, and

$$V_1 = 2\pi\left(\Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!}{(2p-1)\,2^{2p}\,(p!)^2}\frac{a^{2p}P_{2p-2}}{r^{2p-1}}\right),\dagger$$

when $r > a$, with similar expressions for V_2 in terms of r' and Q.

The following relations are true:—

$$(1)\quad \frac{d}{dx}\frac{P_\sigma}{r^{\sigma+1}} = \frac{1}{r^{\sigma+2}}\frac{d}{d\theta}P_{\sigma+1};$$

$$(2)\quad \frac{d}{dx}r^\sigma P_\sigma = r^{\sigma-1}\frac{d}{d\theta}P_{\sigma-1};$$

$$(3)\quad \frac{d}{dz}\frac{P_\sigma}{r^{\sigma+1}} = (\sigma+1)\frac{P_{\sigma+1}}{r^{\sigma+2}};\ddagger$$

$$(4)\quad \frac{d}{dz}r^\sigma P^\sigma = -\sigma r^{\sigma-1}P_{\sigma-1}.\ddagger$$

When Q and r' are substituted for P and r, the forms of (1) and (2) remain unaltered; whilst (3) and (4) become respectively

$$(5)\quad \frac{d}{dz}\frac{Q_\sigma}{r'^{\sigma+1}} = -(\sigma+1)\frac{Q_{\sigma+1}}{r'^{\sigma+2}};$$

$$(6)\quad \frac{d}{dz}r'^\sigma Q_\sigma = \sigma r'^{\sigma-1}Q_{\sigma-1}.$$

* Maxwell's 'Electricity and Magnetism,' Second Edition, vol. 2, § 676, eq. 12.

† *Ibid.*, eqs. 14 and 15.

‡ The formulæ (3) and (4) may be generalised as follows:—

$$\frac{d^i}{dz^i}\frac{P_\sigma}{r^{\sigma+1}} = \frac{(\sigma+i)!}{\sigma!}\frac{P_{\sigma+i}}{r^{\sigma+i+1}},$$

$$\frac{d^i}{dz^i}r^\sigma P_\sigma = (-1)^i\frac{\sigma!}{(\sigma-i)!}r^{\sigma-i}P_{\sigma-i}.$$

These relations may all be proved (as was done originally by the author) by induction, using the well known relation

$$P_\sigma = \frac{2\sigma - 1}{\sigma} \cos\theta \, P_{\sigma-1} - \frac{\sigma - 1}{\sigma} P_{\sigma-2}.$$

Assuming them to be true for $P_{\sigma-1}$ and $P_{\sigma-2}$, they may then be shown to be true for P_σ; and trial establishes the equality in the case of P_2 and P_1. Professor T. R. Lyle has, however, given me a much shorter and neater proof by means of the relations

$$(a) \quad (1-\mu^2) P'_\sigma = \sigma P_{\sigma-1} - \sigma\mu P_\sigma,$$

$$(b) \quad (\sigma+1) P_{\sigma+1} - (2\sigma+1)\mu P_\sigma + \sigma P_{\sigma-1} = 0,$$

$$(c) \quad \mu P'_\sigma = \sigma P_\sigma + P'_{\sigma-1},$$

$$(d) \quad P'_{\sigma+1} - P'_{\sigma-1} = (2\sigma+1) P_\sigma,$$

where, as usual, $P'_\sigma = \dfrac{dP_\sigma}{d\mu}$, $\quad \mu = \cos\theta = \dfrac{l-z}{r}$,

if l denote the half-length of the coil. Since

$$r = \sqrt{(l-z)^2 + x^2},$$

we have
$$\frac{dr}{dz} = -\mu, \qquad \frac{dr}{dx} = \sqrt{1-\mu^2},$$

$$\frac{d\mu}{dz} = \frac{-(1-\mu^2)}{r}, \qquad \frac{d\mu}{dx} = \frac{-\mu\sqrt{1-\mu^2}}{r}.$$

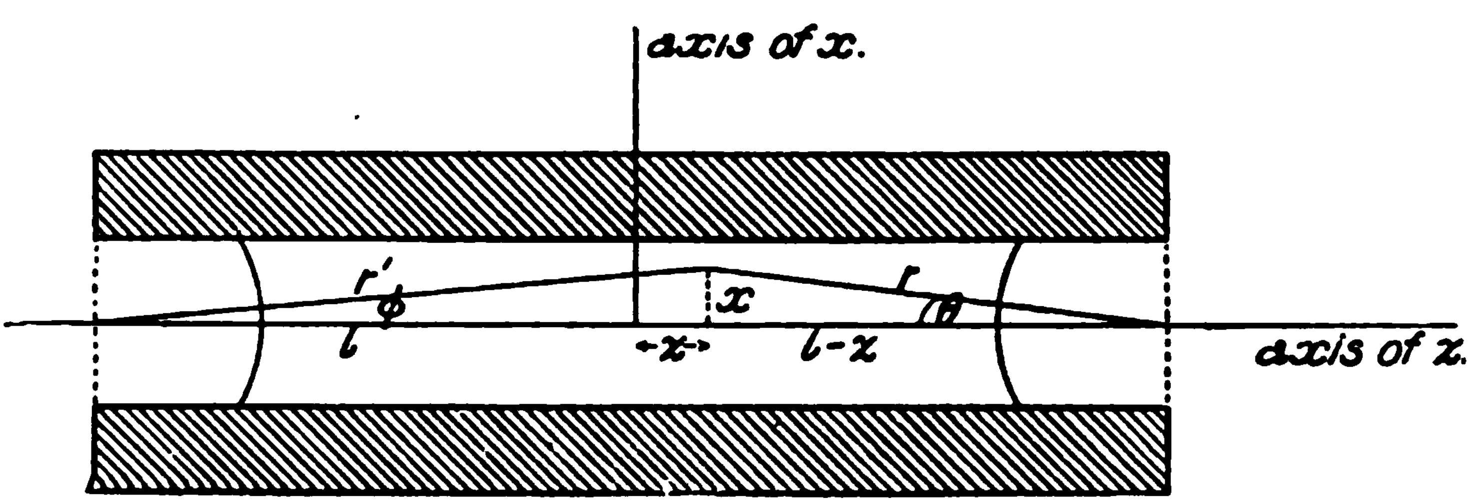
A single example is sufficient. Taking the case of equation (3) we have

$$\frac{d}{dz}\frac{P_\sigma}{r^{\sigma+1}} = \frac{1}{r^{\sigma+2}}\left[(\sigma+1)\mu P_\sigma - (1-\mu^2) P'_\sigma\right]$$

$$= (\sigma+1)\frac{P_{\sigma+1}}{r^{\sigma+2}}, \text{ by } (a) \text{ and } (b).$$

Case I.

Expressions for the components of the magnetic force in that region of a long coil for which r and r' are both greater than S.



It will be found that

(α) X =

$$2\pi\gamma n\left[\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(S^{2p+1}-T^{2p+1})}{(2p+1)(2p-1)\,2^{2p}(p\,!)^2}\left(\frac{1}{r^{2p}}\frac{dP_{2p-1}}{d\theta}-\frac{1}{r'^{2p}}\frac{dQ_{2p-1}}{d\theta}\right)\right];$$

$$(\beta)\quad Z = 2\pi\gamma n\left[2(S-T)\right.$$

$$\left.+\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(S^{2p+1}-T^{2p+1})}{(2p+1)\,2^{2p}(p\,!)^2}\left(\frac{P_{2p-1}}{r^{2p}}+\frac{Q_{2p-1}}{r'^{2p}}\right)\right].$$

Since $n(S-T)$ = the number of turns per unit of length, = N (say), the defect of Z from $4\pi N\gamma$ depends on the importance of the terms under the sign Σ. For the region near the equatorial plane of an infinitely long solenoid these terms become vanishingly small.

The series under the sign Σ are convergent, though not very rapidly so if S is nearly as large as r or r'.

Case II.

Expressions for the region near one end of a long solenoid where $r < T$ and $r' > S$.

In this case

$$(\gamma)\quad X = 2\pi\gamma n\left[\frac{r}{2}\frac{dP_1}{d\theta}\log_e\frac{T}{S}\right.$$

$$+\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p+2)!\left(\frac{1}{S^{2p}}-\frac{1}{T^{2p}}\right)}{2p(2p+1)2^{2p+2}\{(p+1)!\}^2}r^{2p+1}\frac{dP_{2p+1}}{d\theta}$$

$$\left.+\Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!\,(S^{2p+1}-T^{2p+1})}{(2p+1)(2p-1)\,2^{2p}(p\,!)^2}\frac{1}{r'^{2p}}\frac{dQ_{2p-1}}{d\phi}\right];$$

$$(\delta)\quad Z = 2\pi\gamma n\left[S-T+rP_1\log_e\frac{S}{T}\right.$$

$$+\Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!\left(\frac{1}{S^{2p}}-\frac{1}{T^{2p}}\right)}{(2p)\,2^{2p}(p\,!)^2}r^{2p+1}P_{2p+1}$$

$$\left.+\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(S^{2p+1}-T^{2p+1})}{(2p+1)\,2^{2p}(p\,!)^2}\frac{Q_{2p-1}}{r'^{2p}}\right].$$

From (δ) it is easy to deduce the fact that in the plane of one end of an infinitely long solenoid the axial component is constant and equal to $2\pi N\gamma$, *i.e.*, half its value for the equatorial region of the same coil.

Case III.

Short solenoid: expressions for the components in the region in which r and r' are both less than T.

Here

(ϵ) X =

$$2\pi\gamma n\left[\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\left(\frac{1}{S^{2p}}-\frac{1}{T^{2p}}\right)}{2p\,.\,2^{2p+1}\,.\,p!\,(p+1)!}\left(r^{2p+1}\frac{dP_{2p+1}}{d\theta}-r'^{2p+1}\frac{dQ_{2p+1}}{d\phi}\right)\right];$$

$$(\zeta)\quad Z = 2\pi\gamma n\left[2l\log_e\frac{S}{T}\right.$$

$$\left.+\Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!\left(\frac{1}{S^{2p}}-\frac{1}{T^{2p}}\right)}{(2p)\,2^{2p}(p!)^2}(r^{2p+1}P_{2p+1}+r'^{2p+1}Q_{2p+1})\right].$$

It is easily deduced from (ζ) that the value of Z at the centre of a circular coil consisting of a few turns all having approximately the same radius T, is $2\pi\gamma K/T$, where K is the total number of turns.

Case IV.

Expression for the magnetic force in that region of a solenoid in which $S > r > T$ and $r' > S$.

If $\qquad \frac{T}{r} = \epsilon < 1; \qquad \frac{r}{S} = \rho < 1,$

we have

$$X = 2\pi\gamma n\left[\frac{r}{2}\frac{dP}{d\theta}\log_e\rho + r\,\Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!\,(1-\rho^{2p})}{2p.2^{2p+1}\,(p+1)!\,p!}\frac{dP_{2p+1}}{d\theta}\right.$$

$$+r\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(1-\epsilon^{2p+1})}{(2p+1)(2p-1)\,2^{2p}\,(p!)^2}\frac{dP_{2p-1}}{d\theta}$$

$$\left.+\Sigma_{p=1}^{p=\infty}(-1)^{p+1}\frac{(2p)!\,(S^{2p+1}-T^{2p+1})}{(2p+1)(2p-1)2^{2p}(p!)^2}\frac{1}{r'^{2p}}\frac{dQ_{2p-1}}{d\phi}\right];$$

$$Z = 2\pi\gamma n\left[S + r - 2T - rP_1\log_e\rho\right.$$

$$+r\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(1-\rho^{2p})}{(2p)\,2^{2p}\,(p!)^2}P_{2p+1}$$

$$+r\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(1-\epsilon^{2p+1}}{(2p+1)\,2^{2p}\,(p!)^2}P_{2p-1}$$

$$\left.+\Sigma_{p=1}^{p=\infty}(-1)^p\frac{(2p)!\,(S^{2p+1}-T^{2p+1})}{(2p+1)\,2^{2p}\,(p!)^2}\frac{Q_{2p-1}}{r'^{2p}}\right].$$

The cases for which

$$r' < \mathrm{T} \quad \text{and} \quad \mathrm{S} > r > \mathrm{T},$$
$$\text{and } \mathrm{S} > r > \mathrm{T} \quad \text{and} \quad \mathrm{S} > r' > \mathrm{T},$$

respectively, do not need separate investigation. The results, though long and cumbrous, can easily be written down from the foregoing.

The accompanying tables were originally calculated by means of the formulæ:

$$\frac{d\mathrm{P}_0}{d\theta} = 0,$$

$$\frac{d\mathrm{P}_1}{d\theta} = -\sin\theta,$$

$$\frac{d\mathrm{P}_2}{d\theta} = -\tfrac{3}{2}\sin 2\theta,$$

$$\frac{d\mathrm{P}_3}{d\theta} = -6\sin\theta + \tfrac{15}{2}\sin^3\theta,$$

$$\frac{d\mathrm{P}_4}{d\theta} = -5\sin 2\theta + \tfrac{35}{4}\sin^2\theta\sin 2\theta,$$

$$\frac{d\mathrm{P}_5}{d\theta} = -15\sin\theta + \tfrac{105}{2}\sin^3\theta - \tfrac{315}{8}\sin^5\theta,$$

$$\frac{d\mathrm{P}_6}{d\theta} = -\tfrac{21}{2}\sin 2\theta + \tfrac{189}{4}\sin^2\theta\sin 2\theta - \tfrac{693}{16}\sin^4\theta\sin 2\theta,$$

$$\frac{d\mathrm{P}_7}{d\theta} = -28\sin\theta + 189\sin^3\theta - \tfrac{693}{2}\sin^5\theta + \tfrac{3003}{16}\sin^7\theta.$$

The results were finally checked by means of the relation

$$\frac{d\mathrm{P}_{n+1}}{d\theta} = \frac{d\mathrm{P}_{n-1}}{d\theta} - (2n+1)\mathrm{P}_n\sin\theta,$$

using for this purpose the table of P_n calculated by Messrs. Holland, Jones, and Lamb, and published by Professor Perry in the 'Phil. Mag.,' vol. 32, 1891. This is not a complete check on the fourth figure in my tables, but no disagreement larger than 0·0004 has been passed over without examination, and that only in the higher values of $d\mathrm{P}_6/d\theta$. It is therefore hoped that the tables may be found accurate as a whole, though it is perhaps scarcely to be expected but that some errors have escaped detection.

With the aid of Perry's tables and those now given the magnetic force at any point inside a coil may be found numerically by means of the formulæ in the body of the paper. For points outside the coil a

slight alteration of the expressions for Z is necessary; those for X remain the same.

It has been thought sufficient for numerical application to give $dP_7/d\theta$ to three places of decimals. The establishment of the fourth place would involve a considerable amount of extra labour. Though $dP_2/d\theta$, $dP_4/d\theta$, and $dP_6/d\theta$ are not used in the present work, they have been calculated in order to complete the table as far as it goes.

Numerical Values of the Derivatives of the First Seven Zonal Harmonics, at Intervals of One Degree.

$\frac{2\theta}{\pi}$.	$\frac{dP_1}{d\theta}$.	$\frac{dP_2}{d\theta}$.	$\frac{dP_3}{d\theta}$.	$\frac{dP_4}{d\theta}$.	$\frac{dP_5}{d\theta}$.	$\frac{dP_6}{d\theta}$.	$\frac{dP_7}{d\theta}$.	$\frac{2\theta}{\pi}$.
0	0·0000	0·0000	0·0000	0·0000	0·0000	0·0000	0·000	0
1	−0·0175	−0·0523	−0·1047	−0·1744	−0·2615	−0·3659	−0·488	1
2	−0·0349	−0·1046	−0·2091	−0·3480	−0·5213	−0·7284	−0·969	2
3	−0·0523	−0·1568	−0·3129	−0·5201	−0·7775	−1·0841	−1·438	3
4	−0·0698	−0·2088	−0·4160	−0·6901	−1·0286	−1·4295	−1·890	4
5	−0·0872	−0·2605	−0·5180	−0·8570	−1·2728	−1·7614	−2·317	5
6	−0·1045	−0·3119	−0·6186	−1·0197	−1·5085	−2·0768	−2·715	6
7	−0·1219	−0·3629	−0·7176	−1·1781	−1·7341	−2·3727	−3·080	7
8	−0·1392	−0·4135	−0·8148	−1·3315	−1·9481	−2·6465	−3·405	8
9	−0·1564	−0·4635	−0·9099	−1·4789	−2·1492	−2·8954	−3·689	9
10	−0·1736	−0·5130	−1·0026	−1·6199	−2·3360	−3·1174	−3·926	10
11	−0·1908	−0·5619	−1·0928	−1·7537	−2·5074	−3·3104	−4·116	11
12	−0·2079	−0·6101	−1·1801	−1·8799	−2·6621	−3·4729	−4·254	12
13	−0·2250	−0·6576	−1·2643	−1·9978	−2·7998	−3·6034	−4·341	13
14	−0·2419	−0·7042	−1·3453	−2·1069	−2·9181	−3·7008	−4·376	14
15	−0·2588	−0·7500	−1·4229	−2·2069	−3·0178	−3·7646	−4·358	15
16	−0·2756	−0·7949	−1·4968	−2·2973	−3·0978	−3·7943	−4·288	16
17	−0·2924	−0·8388	−1·5668	−2·3777	−3·1576	−3·7899	−4·169	17
18	−0·3090	−0·8817	−1·6328	−2·4478	−3·1970	−3·7518	−4·001	18
19	−0·3256	−0·9235	−1·6946	−2·5073	−3·2158	−3·6806	−3·788	19
20	−0·3420	−0·9642	−1·7520	−2·5560	−3·2141	−3·5774	−3·584	20
21	−0·3584	−1·0037	−1·8050	−2·5937	−3·1920	−3·4435	−3·241	21
22	−0·3746	−1·0420	−1·8534	−2·6203	−3·1497	−3·2804	−2·915	22
23	−0·3907	−1·0790	−1·8970	−2·6357	−3·0877	−3·0917	−2·561	23
24	−0·4067	−1·1147	−1·9357	−2·6400	−3·0067	−2·8749	−2·183	24
25	−0·4226	−1·1491	−1·9696	−2·6330	−2·9073	−2·6371	−1·787	25

Numerical Values of the Derivatives of the First Seven Zonal Harmonics, at Intervals of One Degree—*continued.*

$\frac{2\theta}{\pi}$.	$\frac{dP_1}{d\theta}$.	$\frac{dP_2}{d\theta}$.	$\frac{dP_3}{d\theta}$.	$\frac{dP_4}{d\theta}$.	$\frac{dP_5}{d\theta}$.	$\frac{dP_6}{d\theta}$.	$\frac{dP_7}{d\theta}$.	$\frac{2\theta}{\pi}$.
26	−0·4384	−1·1820	−1·9984	−2·6150	−2·7903	−2·3794	−1·378	26
27	−0·4540	−1·2135	−2·0222	−2·5861	−2·6568	−2·1046	−0·963	27
28	−0·4695	−1·2436	−2·0408	−2·5464	−2·5077	−1·8156	−0·548	28
29	−0·4848	−1·2721	−2·0542	−2·4961	−2·3443	−1·5155	−0·136	29
30	−0·5000	−1·2990	−2·0625	−2·4357	−2·1680	−1·2077	+0·263	30
31	−0·5150	−1·3244	−2·0654	−2·3654	−1·9799	−0·8953	+0·647	31
32	−0·5299	−1·3482	−2·0635	−2·2855	−1·7817	−0·5815	+1·010	32
33	−0·5446	−1·3703	−2·0562	−2·1966	−1·5748	−0·2697	+1·347	33
34	−0·5592	−1·3908	−2·0437	−2·0991	−1·3608	+0·0370	+1·654	34
35	−0·5736	−1·4095	−2·0262	−1·9934	−1·1413	+0·3354	+1·927	35
36	−0·5878	−1·4266	−2·0036	−1·8802	−0·9179	+0·6225	+2·162	36
37	−0·6018	−1·4419	−1·9761	−1·7600	−0·6924	+0·8955	+2·357	37
38	−0·6157	−1·4554	−1·9438	−1·6334	−0·4664	+1·1516	+2·510	38
39	−0·6293	−1·4672	−1·9066	−1·5011	−0·2415	+1·3885	+2·620	39
40	−0·6428	−1·4772	−1·8648	−1·3637	−0·0194	+1·6038	+2·684	40
41	−0·6561	−1·4854	−1·8185	−1·2219	+0·1983	+1·7955	+2·705	41
42	−0·6691	−1·4918	−1·7678	−1·0764	+0·4100	+1·9620	+2·681	42
43	−0·6820	−1·4963	−1·7129	−0·9279	+0·6142	+2·1017	+2·614	43
44	−0·6947	−1·4991	−1·6539	−0·7772	+0·8095	+2·2136	+2·506	44
45	−0·7071	−1·5000	−1·5910	−0·6250	+0·9943	+2·2969	+2·359	45
46	−0·7193	−1·4991	−1·5244	−0·4720	+1·1677	+2·3510	+2·176	46
47	−0·7314	−1·4963	−1·4542	−0·3190	+1·3282	+2·3757	+1·961	47
48	−0·7431	−1·4918	−1·3808	−0·1668	+1·4749	+2·3715	+1·717	48
49	−0·7547	−1·4854	−1·3042	−0·0160	+1·6067	+2·3382	+1·449	49
50	−0·7660	−1·4772	−1·2248	+0·1327	+1·7229	+2·2772	+1·161	50

$\frac{2\theta}{\pi}$.	$\frac{dP_1}{d\theta}$.	$\frac{dP_2}{d\theta}$.	$\frac{dP_3}{d\theta}$.	$\frac{dP_4}{d\theta}$.	$\frac{dP_5}{d\theta}$.	$\frac{dP_6}{d\theta}$.	$\frac{dP_7}{d\theta}$.	$\frac{2\theta}{\pi}$.
51	−0·7771	−1·4672	−1·1427	+0·2784	+1·8225	+2·1892	+0·859	51
52	−0·7880	−1·4554	−1·0581	+0·4205	+1·9053	+2·0760	+0·546	52
53	−0·7986	−1·4419	−0·9714	+0·5584	+1·9704	+1·9387	+0·230	53
54	−0·8090	−1·4266	−0·8828	+0·6914	+2·0173	+1·7797	−0·088	54
55	−0·8191	−1·4095	−0·7925	+0·8188	+2·0474	+1·6009	−0·399	55
56	−0·8290	−1·3908	−0·7007	+0·9401	+2·0587	+1·4046	−0·700	56
57	−0·8387	−1·3703	−0·6078	+1·0546	+2·0521	+1·1936	−0·986	57
58	−0·8480	−1·3482	−0·5140	+1·1620	+2·0281	+0·9699	−1·252	58
59	−0·8572	−1·3244	−0·4196	+1·2617	+1·9866	+0·7369	−1·495	59
60	−0·8660	−1·2990	−0·3248	+1·3532	+1·9283	+0·4973	−1·711	60
61	−0·8746	−1·2721	−0·2299	+1·4361	+1·8538	+0·2540	−1·896	61
62	−0·8829	−1·2436	−0·1351	+1·5101	+1·7640	+0·0099	−2·048	62
63	−0·8910	−1·2135	−0·0409	+1·5748	+1·6596	−0·2321	−2·165	63
64	−0·8988	−1·1820	+0·0527	+1·6306	+1·5420	−0·4691	−2·244	64
65	−0·9063	−1·1491	+0·1454	+1·6755	+1·4114	−0·6983	−2·286	65
66	−0·9135	−1·1147	+0·2368	+1·7111	+1·2698	−0·9170	−2·290	66
67	−0·9205	−1·0790	+0·3268	+1·7366	+1·1187	−1·1226	−2·255	67
68	−0·9272	−1·0420	+0·4149	+1·7520	+0·9580	−1·3129	−2·183	68
69	−0·9336	−1·0087	+0·5011	+1·7574	+0·7906	−1·4855	−2·076	69
70	−0·9397	−0·9642	+0·5851	+1·7525	+0·6173	−1·6386	−1·984	70
71	−0·9455	−0·9235	+0·6666	+1·7376	+0·4397	−1·7704	−1·762	71
72	−0·9511	−0·8817	+0·7455	+1·7131	+0·2594	−1·8794	−1·561	72
73	−0·9563	−0·8388	+0·8214	+1·6787	+0·0776	−1·9646	−1·334	73
74	−0·9613	−0·7949	+0·8941	+1·6349	−0·1037	−2·0248	−1·088	74
75	−0·9659	−0·7500	+0·9636	+1·5519	−0·2833	−2·0596	−0·824	75

Numerical Values of the Derivatives of the First Seven Zonal Harmonics, at Intervals of One Degree—*continued.*

$\frac{2\theta}{\pi}$.	$\frac{dP_1}{d\theta}$.	$\frac{dP_2}{d\theta}$.	$\frac{dP_3}{d\theta}$.	$\frac{dP_4}{d\theta}$.	$\frac{dP_5}{d\theta}$.	$\frac{dP_6}{d\theta}$.	$\frac{dP_7}{d\theta}$.	$\frac{2\theta}{\pi}$.
76	−0·9703	−0·7042	+1·0295	+1·5200	−0·4595	−2·0687	−0·548	76
77	−0·9744	−0·6576	+1·0918	+1·4498	−0·6309	−2·0520	−0·264	77
78	−0·9781	−0·6101	+1·1501	+1·3714	−0·7961	−2·0098	+0·023	78
79	−0·9816	−0·5619	+1·2044	+1·2854	−0·9537	−1·9428	+0·309	79
80	−0·9848	−0·5130	+1·2545	+1·1923	−1·1023	−1·8519	+0·589	80
81	−0·9877	−0·4635	+1·3003	+1·0926	−1·2407	−1·7382	+0·858	81
82	−0·9903	−0·4135	+1·3416	+0·9870	−1·3679	−1·6032	+1·112	82
83	−0·9925	−0·3629	+1·3783	+0·8758	−1·4831	−1·4487	+1·348	83
84	−0·9945	−0·3119	+1·4103	+0·7598	−1·5841	−1·2760	+1·559	84
85	−0·9962	−0·2605	+1·4376	+0·6396	−1·6715	−1·0881	+1·745	85
86	−0·9976	−0·2088	+1·4599	+0·5160	−1·7440	−0·8868	+1·901	86
87	−0·9986	−0·1568	+1·4774	+0·3895	−1·8009	−0·6747	+2·024	87
88	−0·9994	−0·1046	+1·4899	+0·2608	−1·8420	−0·4544	+2·115	88
89	−0·9998	−0·0523	+1·4975	+0·1308	−1·8667	−0·2286	+2·169	89
90	−1·0000	0·0000	+1·5000	0·0000	−1·8750	0·0000	+2·187	90

"Tables for the Solution of the Equation

$$\frac{d^2y}{dx^2}+\frac{1}{x}\cdot\frac{dy}{dx}-\left(1+\frac{n^2}{x^2}\right)y = 0."$$

By W. STEADMAN ALDIS, M.A. Communicated by Professor J. J. THOMSON, F.R.S. Received and Read June 16, 1898.

1. The object of the present paper is to exhibit the processes of calculation of the values of the two solutions of the equation

$$\frac{d^2y}{dx^2}+\frac{1}{x}\cdot\frac{dy}{dx}-\left(1+\frac{n^2}{x^2}\right)y = 0 \quad\text{..................}\quad (1),$$

for successive values of x in the two cases of $n = 0$ and $n = 1$.

That is if

$$y = AI_n(x)+BK_n(x)$$

be the complete integral of (1), where $K_n(x)$ is a function which becomes zero when x is indefinitely increased, our object is to calculate the values of $I_0(x)$, $I_1(x)$, $K_0(x)$, $K_1(x)$ for successive equidistant values of x.

The values of $I_0(x)$ and $I_1(x)$ have been calculated and published by a committee of the British Association for the Advancement of Science. To the best of the writer's knowledge, no steps have been taken towards the computation of $K_0(x)$ and $K_1(x)$. The Tables I and II, at the end of this paper, give the values of these latter functions for intervals of 0·1 in the argument, to such a large number of decimal places as will make it a mere matter of difference calculation to determine intermediate values of $K_0(x)$ and $K_1(x)$ to any reasonable degree of accuracy, at any rate for values of x greater than unity; and also by means of the sequence laws to derive those of $K_2(x)$, $K_3(x)$ ......, as far as may be requisite.

It will be convenient to state a number of well-known theorems in regard to the solution of (1).

2. The function $I_n(x)$ is defined by the condition

$$I_n(x) = \sum_{r=0}^{r=\infty}\frac{\left(\frac{x}{2}\right)^{n+2r}}{\Pi(r)\,\Pi(n+r)} \quad\text{..................}\quad (2).$$

For all values of n

$$y = AI_n(x) \quad\text{..............................}\quad (3)$$

is a solution of (1), $\Pi(n)$ being the function defined by Gauss.*

* 'Werke,' vol. 3, p. 145.

When n is a positive integer, a second solution is given by

$$y = \mathrm{B}\Lambda_n(x) \quad \text{.............................} \quad (4),$$

where

$$\Lambda_n(x) = \mathrm{I}_n(x)\log x$$
$$+\frac{(-2)^{n-1}\Pi(n-1)}{x^n}\left\{1-\frac{\left(\frac{x}{2}\right)^2}{1\,.\,\overline{n-1}}+\frac{\left(\frac{x}{2}\right)^4}{1\,.\,2\,.\,\overline{n-1}\,\overline{n-2}}-\ldots\ldots\right.$$
$$\left.+\frac{(-1)^{n-1}\left(\frac{x}{2}\right)^{2n-2}}{\Pi(n-1)\,.\,\Pi(n-1)}\right\}$$
$$-\tfrac{1}{2}\sum_{r=0}^{r=\infty}\frac{\left(\frac{x}{2}\right)^{n+2r}}{\Pi(n)\,\Pi(n+r)}(\mathrm{S}_r+\mathrm{S}_{n+r}) \quad \text{..........................} \quad (5),$$

where S_r denotes the series $1+\frac{1}{2}+\frac{1}{3}+\ldots\ldots+\frac{1}{r}$. It is also necessary to assume zero as the value of the, in itself meaningless, symbol S_0.

It is also known that if E represent the quantity

$$\log 2+\frac{\Gamma'(1)}{\Gamma(1)}$$

the function $\mathrm{EI}_n(x)-\Lambda_n(x)$ becomes indefinitely small as x increases indefinitely. Hence since in virtue of (3) and (4)

$$y = \mathrm{B}\{\mathrm{EI}_n(x)-\Lambda_n(x)\}$$

is obviously a solution of (1), it is allowable to write

$$\mathrm{K}_n(x) = \mathrm{EI}_n(x)-\Lambda_n(x) \quad \text{...................} \quad (6).$$

3. The three functions I, Λ, and K are all subject to the laws

$$\left.\begin{aligned}\frac{d}{dx}(x^{-n}\mathrm{I}_n) &= x^{-n}\mathrm{I}_{n+1}\\ \frac{d}{dx}(x^n\mathrm{I}_n) &= x^n\mathrm{I}_{n-1}\end{aligned}\right\}(\alpha),$$

where I_n is written for $\mathrm{I}_n(x)$. These equations hold when either Λ or K is substituted for I. When n has the value zero, the two equations must be replaced by the single equation

$$\frac{d\mathrm{I}_0}{dx} = \mathrm{I}_1,$$

or the same with Λ or K written for I.

These laws give, for values of n not less than unity,

$$\left.\begin{aligned} 2\frac{dI_n}{dx} &= I_{n+1}+I_{n-1} \\ \frac{2nI_n}{x} &= I_{n-1}-I_{n+1} \end{aligned}\right\} (\beta).$$

They are known and will be quoted as the sequence laws.

4. It can be shown that $K_n(x)$ is expressible in two ways in terms of a definite integral, namely,

$$K_n(x) = (-1)^n \frac{\Gamma(\frac{1}{2})}{\Gamma(n+\frac{1}{2})}\left(\frac{x}{2}\right)^n \int_1^\infty \epsilon^{-px}(p^2-1)^{\frac{2n-1}{2}}dp \;\ldots\; (7),$$

$$K_n(x) = (-1)^n \frac{\Gamma(n+\frac{1}{2})}{\Gamma(\frac{1}{2})}\cdot\left(\frac{2}{x}\right)^n \int_0^\infty \frac{\cos px dp}{(1+p^2)^{\frac{2n+1}{2}}} \;\ldots\ldots\ldots\; (8).$$

By putting $p = 1+\frac{z}{x}$ in (7), expanding the binomial and integrating the separate terms, another form can be obtained for $K_n(x)$, namely,

$$K_n(x) = (-1)^n\left(\frac{\pi}{2x}\right)^{\frac{1}{2}}\epsilon^{-x}\left\{1+\frac{4n^2-1}{8x}+\frac{(4n^2-1)(4n^2-9)}{1\cdot 2\,(8x)^2}+\ldots\ldots\right\} (9);$$

where the series within the bracket can be brought to a close at any point by means of a remainder term, which, after a certain point in the series, is always numerically less than the next term given by the general law of the series.

5. It is now possible to explain the processes by which the Tables I and II at the end of this paper have been calculated. The series actually employed are, for the smaller values of x, the ultimately convergent series (6); and for larger values the series (9).

In the calculation of the Λ functions, the natural logarithms of x are required. These the writer has taken from Wolfram's table at the end of Vega's 'Thesaurus Logarithmorum,' having, in the numbers up to 20 and for the prime numbers up to 59, verified them to 30 places of decimals by calculation.

The quantity E has been derived from Gauss.*

Using Gauss's notation

$$\psi(z) = \frac{\Pi'(z)}{\Pi(z)} = \frac{\Gamma'(z+1)}{\Gamma(z+1)}$$

it follows that $\quad E = \log 8+\psi(-\frac{1}{2}) = \log 2+\psi(0).$

* 'Werke,' vol. 3, p. 155.

From Wolfram's table, taking thirty-six places,

$$\log 2 = 0{\cdot}693\ 147\ 180\ 559\ 945\ 309\ 417\ 232\ 121\ 458\ 176\ 568.$$

The value of $\psi(0)$ is given in a note by Gauss as

$$\psi(0) = -0{\cdot}577\ 215\ 664\ 901\ 532\ 860\ 606\ 512\ 090\ 082\ 402\ 431.$$

The algebraical sum of these is

$$0{\cdot}115\ 931\ 515\ 658\ 412\ 448\ 810\ 720\ 031\ 375\ 774\ 137,$$

which is, therefore, the value of E to many more places than will be required.

The quantity $-\psi(0)$ is, of course, Euler's constant, and the above value is also to be derived from a paper by the late Professor J. C. Adams in the 'Proceedings of the Royal Society.'

6. The calculations of $I_0(x)$, $I_1(x)$, $K_0(x)$, $K_1(x)$ are best carried on in connection with one another. We have

$$K_0(x) = -I_0(x)\{\log x - E\} + \left\{\left(\frac{x}{2}\right)^2 + \left(\frac{x}{2}\right)^4 \frac{S_2}{\{\Pi(2)\}^2} + \left(\frac{x}{2}\right)^6 \cdot \frac{S_3}{\{\Pi(3)\}^2} + \ldots\ldots\right\}.$$

$$K_1(x) = -I_1(x)\{\log x - E\} - \frac{1}{x} + \tfrac{1}{2}\left\{\left(\frac{x}{2}\right) + \left(\frac{x}{2}\right)^3 \frac{S_1 + S_2}{\Pi(1).\Pi(2)} + \ldots\ldots\right\}.$$

The first process is to find the values of $I_0(x)$ and $I_1(x)$.

If a series of quantities, β_0, β_1, $\beta_2, \ldots\ldots$, β_{2r}, $\beta_{2r+1}, \ldots\ldots$ be determined by the successive relations

$$\beta_{2r+1} = \frac{\frac{1}{2}x}{r+1} \cdot \beta_{2r}, \qquad \beta_{2r+2} = \frac{\frac{1}{2}x}{r+1} \cdot \beta_{2r+1},$$

coupled with the condition $\beta_0 = 1$, it is easily seen that

$$I_0(x) = \beta_0 + \beta_2 + \beta_4 + \ldots\ldots = \sum_{r=0}^{r=\infty} \beta_{2r},$$

$$I_1(x) = \beta_1 + \beta_3 + \beta_5 + \ldots\ldots = \sum_{r=0}^{r=\infty} \beta_{2r+1}.$$

Thus the successive terms of $I_0(x)$ and $I_1(x)$ are obtained by multiplying by a series of factors of the form $\frac{1}{2}x/r+1$; the alternate terms when obtained are written down underneath one another, the odd ones in one column, the even ones in another, and by addition of each column the values of $I_0(x)$ and $I_1(x)$ are obtained.

In working out the values of $I_0(x)$ and $I_1(x)$ given in Table I, all the

multiplications by $\frac{1}{2}x/r+1$ have been conducted in two different forms to avoid the possibility of mistakes. Thus, for instance, in working out $I_0(5\cdot2)$ and $I_1(5\cdot2)$, the factor $\frac{1}{2}x/8$, or $2\cdot6/8$, can be used as it stands, or as $\frac{13}{40}$, and also put into the form $\frac{1}{8}+\frac{1}{5}$. The adoption in all cases of two quite different processes is an almost infallible guide to the detection of a mistake.

7. The values of $I_0(x)$ and $I_1(x)$ being thus obtained, $K_0(x)$ and $K_1(x)$ can be derived..

We have

$$K_0(x) = -I_0(x)\{\log x - E\} + \left\{\left(\frac{x}{2}\right)^2 + \left(\frac{x}{2}\right)^4\frac{S_2}{\{\Pi(2)\}^2} + \left(\frac{x}{2}\right)^6\frac{S_3}{\{\Pi(3)\}^2} + \dots\right\}$$

$$= -I_0(x)\{\log x - E\} + \{\beta_2 + \beta_4 S_2 + \beta_6 S_3 + \beta_8 S_4 + \dots\}.$$

But $0 = I_0(x) - 1 - \{\beta_2 + \beta_4 + \beta_6 + \beta_8 + \dots\dots\}.$

Adding

$$K_0(x) = -I_0(x)\{\log x - E - 1\} - 1 + \{\beta_4(S_2-1) + \beta_6(S_3-1) + \beta_8(S_4-1) + \dots\}.$$

It will be convenient to denote $\beta_{2r}(S_r-1)$ by the symbol γ_{2r}. Hence

$$K_0(x) = -I_0(x)\{\log x - E - 1\} - 1 + \{\gamma_4 + \gamma_6 + \gamma_8 + \dots\dots\} \quad \dots\dots\dots \quad (10)$$

The value of $I_0(x)$ is known and that of $\log x$ can be found from Wolfram's Table. The quantities γ_{2r} must be derived, each from the corresponding β_{2r}.

For earlier values of γ_{2r} the multiplier $S_r - 1$ is most easily used in the natural form

$$\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \dots\dots + \frac{1}{r}.$$

For later values, it is simpler to use the decimalized values of $S_r - 1$ given in Table V.

In using the primary form, many simplifications are possible, thus $\frac{1}{2}+\frac{1}{3} = \frac{5}{6} = \frac{10}{12}$, and the multiplication is effected by shifting the decimal point one place to the right, and dividing by 12.

Again,
$$\tfrac{1}{2}+\tfrac{1}{3}+\tfrac{1}{4} = 1+\tfrac{1}{12},$$
$$\tfrac{1}{2}+\tfrac{1}{3}+\tfrac{1}{4}+\tfrac{1}{5} = 1+\tfrac{1}{12}+\tfrac{1}{5} = 1+\tfrac{1}{3}-\tfrac{1}{20};$$
$$\tfrac{1}{2}+\tfrac{1}{3}+\tfrac{1}{6} = 1,$$
$$\tfrac{1}{4}+\tfrac{1}{5}+\tfrac{1}{10} = \tfrac{1}{2}+\tfrac{1}{20},$$
$$\tfrac{1}{2}+\tfrac{1}{4}+\tfrac{1}{6}+\tfrac{1}{12} = 1,$$

and so on. In the computation of the values given in Table I, tw-

different processes for computing each γ have been employed, so that any mistake is almost certain to have been detected.

For the lower values of x a second process of calculating the values of γ_{2r} has been found from the obvious fact that if γ'_{2r} be the value of γ_{2r} when x becomes x/m,

$$\gamma'_{2r} = \frac{\gamma_{2r}}{m^{2r}}.$$

Thus the values of the quantities γ for $x = 2{\cdot}6$ can be deduced from those for $x = 5{\cdot}2$ by a series of divisions by 2 or powers of 2.

For the values of x from $x = 3{\cdot}1$ upwards, this process was not available, but either two different transformations of the sum of the vulgar fractions, or one such transformation, and the decimalized value have been used in every case.

Another process, which has been occasionally used, when the fraction $\frac{1}{2}x/r+1$ happened to be in low terms, is based on the easily proved formula

$$\gamma_{2r+2} = \left(\frac{\frac{x}{2}}{r+1}\right)^2 \cdot \gamma_{2r} + \frac{\beta_{2r+2}}{r+1}.$$

It only remains to multiply $I_0(x)$ by $(\log x - E - 1)$ and, adding unity to this product, to subtract the sum from $\Sigma\gamma$. The value of $K_0(x)$, which is always a positive quantity, is then obtained.

8. The second function $K_1(x)$ can be readily expressed in terms of quantities already found.

For

$$K_1(x) = -I_1(x)(\log x - E) - \frac{1}{x} + \frac{1}{2}\left\{\frac{x}{2} + \left(\frac{x}{2}\right)^3 \frac{S_1+S_2}{\Pi(1)\Pi(2)} + \left(\frac{x}{2}\right)^5 \frac{S_2+S_3}{\Pi(2)\Pi(3)} + \ldots\ldots\right\},$$

also

$$0 = I_1(x) - \left\{\frac{x}{2} + \left(\frac{x}{2}\right)^3 \frac{1}{\Pi(1)\Pi(2)} + \left(\frac{x}{2}\right)^5 \frac{1}{\Pi(1)\Pi(2)} + \ldots\ldots\right\},$$

adding

$$K_1(x) = -I_1(x)(\log x - E - 1) - \frac{1}{x} - \frac{x}{4}$$
$$+ \frac{1}{2}\left\{\left(\frac{x}{2}\right)^3 \frac{S_1+S_2-2}{\Pi(1)\Pi(2)} + \ldots\ldots + \left(\frac{x}{2}\right)^{2r+1} \frac{S_r+S_{r+1}-2}{\Pi(r)\Pi(r+1)} + \ldots\ldots\right\};$$

but

$$\left(\frac{x}{2}\right)^3 \frac{S_1+S_2-2}{\Pi(1)\Pi(2)} = \frac{1}{2}\left(\frac{x}{2}\right)^3 \frac{1}{\Pi(1)\Pi(2)} = \frac{2}{x} \cdot \beta_4$$

$$\left(\frac{x}{2}\right)^5 \frac{S_2+S_3-2}{\Pi(2)\Pi(3)} = \left(\frac{x}{2}\right)^5 \cdot \frac{2(S_2-1)+\frac{1}{3}}{\Pi(2)\Pi(3)} = \frac{x}{3}\gamma_4 + \frac{2}{x}\beta_6$$

....

$$\left(\frac{x}{2}\right)^{2r+1}\frac{S_r+S_{r+1}-2}{\Pi(r)\Pi(r+1)} = \left(\frac{x}{2}\right)^{2r+1}\frac{2(S_r-1)+\dfrac{1}{r+1}}{\Pi(r)\Pi(r+1)}$$

$$= \frac{x}{r+1}\gamma_{2r}+\frac{2}{x}\beta_{2r+2}$$

....

Hence

$$\begin{aligned} K_1(x) &= -I_1(x)(\log x - E - 1) - \frac{1}{x} - \frac{x}{4} \\ &\quad + \frac{1}{x}(\beta_4+\beta_6+\ldots\ldots+\beta_{2r+2}+\ldots\ldots) \\ &\quad + \frac{x}{2}\left\{\tfrac{1}{3}\gamma_4+\tfrac{1}{4}\gamma_6+\ldots\ldots+\frac{1}{r+1}\gamma_{2r}\ldots\ldots\right\} \\ &= -I_1(x)(\log x - E - 1) - \frac{1}{x} - \frac{x}{4} + \frac{1}{x}\{I_0(x) - 1 - \beta_2\} \\ &\quad + \frac{x}{2}\left\{\tfrac{1}{3}\gamma_4+\tfrac{1}{4}\gamma_6+\ldots\ldots+\frac{1}{r+1}\gamma_{2r}+\ldots\ldots\right\} \\ &= -I_1(x)(\log x - E - 1) - \frac{2}{x} - \frac{x}{2} + \frac{I_0(x)}{x} \\ &\quad + \frac{x}{2}\left\{\tfrac{1}{3}\gamma_4+\tfrac{1}{4}\gamma_6+\ldots\ldots+\frac{1}{r+1}\gamma_{2r}+\ldots\ldots\right\} \quad \ldots\ldots\ldots\ldots\ (11) \end{aligned}$$

The calculation therefore only involves two operations which entail much labour, namely, the multiplication of $I_1(x)$ by $\{\log x - E - 1\}$ and the computation of the series.

$$\tfrac{1}{3}\gamma_4+\tfrac{1}{4}\gamma_6+\ldots\ldots+\frac{1}{r+1}\gamma_{2r}+\ldots\ldots$$

9. The quantities given in Table I have been computed by these formulæ. The multiplications of $I_0(x)$ and $I_1(x)$ by $(\log x - E - 1)$ have been worked, taking $(\log x - E - 1)$ as multiplicand, which saves a good deal of labour, as many of the lines of multiplication used in finding $K_0(x)$ occur again in finding $K_1(x)$. In all cases the multiplications have been carried to several places further than are used, or given, in the final results.

10. A process of verification has been applied to the values given in Table I, based on the following theorem.

It is easy to show that if $y = y_1$ and $y = y_2$ be any two different solutions of the fundamental equation (1),

$$y_1\frac{dy_2}{dx} - y_2\frac{dy_1}{dx} = \frac{A}{x}.$$

In the particular case of $n = 0$, y_1 and y_2 may have the values $I_0(x$ and $K_0(x)$ respectively. Hence

$$I_0()\frac{dK_0(x)}{dx} - K_0(x)\frac{dI_0(x)}{dx} = \frac{A}{x},$$

or, by means of the sequence laws,

$$I_0(x)K_1(x) - K_0(x)I_1(x) = \frac{A}{x}.$$

Multiplying by x, and putting $x = 0$, it is easily seen that $A = -1$, consequently for all values of x,

$$I_0(x) \,.\, K_1(x) - I_1(x) \,.\, K_0(x) = -\frac{1}{x}.$$

The writer has to thank his friend Captain Makgill, R.E., of Waiuku, Auckland, for verifying by this formula most of the results obtained before the writer left New Zealand. For the others he has had to rely on his own verification.

The formula is an infallible indicator of any mistake in the values of β_s or γ_{2s}, or in the process of multiplication of $I_0(x)$ and $I_1(x)$ into $(\log x - E - 1)$. It obviously will not indicate an erroneous value of this last quantity. The values of $(\log x - E - 1)$ have been all calculated in two different ways, so as to avoid the possibility of mistake, but in order to give the greatest security, a table of the values employed is appended, and the writer hopes that if any mistake is detected, information of it may be sent to him, as it would be a very easy matter to supply the requisite correction to the values of $K_0(x)$ and $K_1(x)$.

As a final test of the accuracy of the results, the differences of the column for $K_0(x)$ have been calculated up to those of the seventeenth order. Up to this point they present in each set of differences a series of regularly decreasing quantities. In the differences of the eighteenth order this ceases to be the case with regard to the quantities at the lower end of the column. This is due to the accumulation of the effect of residual error in the last figures of the column of values of $K_0(x)$. The differences of the seventeenth order at the lower end of the column are quantities consisting of fifteen ciphers followed by six significant figures. Now since 2^{20} is greater than a million, it follows that a residual error of four-tenths of a unit in the last figure, in opposite directions in two consecutive values of $K_0(x)$ might possibly, after eighteen differentiations, produce an error of a unit in the sixth place from the end, consequently completely disorganise the sequence of the eighteenth differences which consist only of five figures. That this has actually happened in this case the writer has shown by examining the effect of *adding to the* values of $K_0(x)$ given in Table I the three additional

figures, two of them certainly correct, which he has calculated. The differences at the lower end of the table then become regular up to the twentieth order.

This process has not been applied to the $K_1(x)$ column, because the writer believes that, granted $K_0(x)$ correct, the verification formula above sufficiently proves the accuracy of $K_1(x)$. The values of the quantities in Table I are believed to be correct to the last figure given. A dot after the last figure indicates that it has been increased by unity, the first figure omitted being equal to or greater than 5.

11. Table II has been computed by means of the formula (9).

The remainder after s terms in the series involves the integral—

$$\int_0^\infty z^{n+s-\frac{1}{2}}\epsilon^{-z}\left(1+\frac{\theta z}{2x}\right)^{n-s-\frac{1}{2}}dz,$$

where θ is some proper fraction.

Now whatever n may be, after a time $n-s-\frac{1}{2}$ becomes negative. When s has reached such a value, inspection of (9) shows that the terms in the series thereafter are alternately positive and negative, inasmuch as a new negative factor is introduced in forming each successive coefficient. It is also evident that, from and after that point in the series, the quantity $\left(1+\frac{\theta z}{2x}\right)^{\frac{2n-2s-1}{2}}$ is numerically less than unity, and the remainder required at any point to give the value of $K_n(x)$ is numerically less than the next term in the series.

Consequently, after the alternation of signs has begun, the sums of s terms, $(s+1)$ terms, $(s+2)$ terms, &c., will be a series of quantities alternately greater and less than the value of $K_n(x)$. As long as the terms of the series diminish, it is possible in this way to obtain a set of quantities, continually approaching one another, between alternate pairs of which $K_n(x)$ must lie.

For the values $n=0$, $n=1$, (9) gives—

$$K_0x = \left(\frac{\pi}{2x}\right)^{\frac{1}{2}}\epsilon^{-x}\left\{1-\frac{1}{8x}+\frac{1\cdot9}{8\cdot16x^2}-\frac{1\cdot9\cdot25}{8\cdot16\cdot24x^3}+\ldots\ldots\right\} \quad \ldots (12)$$

$$K_1(x) = -\left(\frac{\pi}{2x}\right)^{\frac{1}{2}}\epsilon^{-x}\left\{1+\frac{3}{8x}-\frac{3\cdot5}{8\cdot16x^2}+\frac{3\cdot5\cdot21}{8\cdot16\cdot24x^3}-\ldots\ldots\right\} \quad \ldots (13)$$

12. In $K_0(x)$ the alternation of signs begins with the first term. Hence the sum of 1, 3, 5, ... terms is numerically greater than the value of $K_0(x)$, while the sum of 2, 4, 6, ... terms is less.

The $\overline{r+1}$th term is derived from the rth by multiplying by $(2r-1)^2/8rx$. As long as this factor is less than unity, the $\overline{r+1}$th term is less than the rth, and the terms continue to diminish. The $\overline{r+1}$th

term is least when r has the largest value, which makes $(2r-1)^2$ less than $8rx$. This gives $r = q$, where q is the integral part of

$$\tfrac{1}{2}\{2x+1+2(x^2+x)^{\frac{1}{2}}\}.$$

Hence the nearest approach of the limits, within which (12) confines the value of $K_0(x)$, is

$$= \left(\frac{\pi}{2x}\right)^{\frac{1}{2}} \epsilon^{-x} \frac{1\cdot 9\cdot 25 \ldots (2q-1)^2}{\Pi(q)(8x)^q} \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (14)$$

It is evident that as $K_0(x)$ lies between the sum of q terms, and the sum of $q+1$ terms, the mean of these two sums is as near an approximation to the actual value of $K_0(x)$ as (12) will give. This mean cannot differ from $K_0(x)$ by quite half the quantity (14).

If x be an integer, the value of q is $2x$; thus, if $x = 1$ the third term is the smallest: when $x = 5$ the eleventh, when $x = 8$ the seventeenth, and so on. The limit of error, estimated by half the least term, is for $x = 1$, 0·0162; for $x = 2$, 0·0042; for $x = 5$, 0·000 000 022; and for larger values of x the limit becomes rapidly smaller.

For values of x as great as, or greater than, five, $K_0(x)$ can thus be determined with accuracy to seven or more places of decimals.

Very similar statements can be made with respect to the determination of $K_1(x)$ from (13).

13. From (12)

$$K_0(x) = \left(\frac{\pi}{2x}\right)^{\frac{1}{2}} \epsilon^{-x} \left\{1 - \frac{1}{8x} + \frac{1\cdot 9}{1\cdot 2(8x)^2} \ldots\ldots \right\}$$

The multipliers, disregarding the sign, by which the coefficients of the powers of x within the bracket are derived, each from the preceding, are

$$\tfrac{1}{8}, \tfrac{9}{16}, \tfrac{25}{24}, \tfrac{49}{32}, \tfrac{81}{40}, \tfrac{121}{48}, \tfrac{169}{56}, \ldots\ldots$$

Let these numbers be denoted by the symbols $m_1, m_2, m_3, \ldots\ldots$, and let $(\pi/2x)^{\frac{1}{2}}\epsilon^{-x}$ be called β_0. Then if a series of quantities $\beta_1, \beta_2, \beta_3, \ldots\ldots$, be derived by the successive relations

$$\beta_1 = m_1\beta_0 x^{-1}, \quad \beta_2 = m_2\beta_1 x^{-1}, \quad \beta_3 = m_3\beta_2 x^{-1}, \ldots\ldots\ldots\ldots \quad (15)$$

it is evident that

$$\begin{aligned} K_0(x) &= \{\beta_0 - \beta_1 + \beta_2 - \beta_3 + \beta_4 \ldots\ldots\} \\ &= (\beta_0 + \beta_2 + \beta_4 + \ldots\ldots) - (\beta_1 + \beta_3 + \beta_5 + \ldots\ldots) \end{aligned}$$

The relations (15) are adapted to logarithmic computation. For the value of β_0 two logarithms beside that of x are required. These are

$$\log \epsilon = 0\cdot 434\ 2944\ 819;$$

$$\log\left(\frac{\pi}{2}\right)^{\frac{1}{2}} = 0\cdot 098\ 0599\ 325.$$

With the help of these and the logarithm of x, that of β_0 can be easily ascertained, and then, if the logarithms of $m_1, m_2, m_3, \ldots\ldots$, be tabulated, it is easy to derive those of $\beta_1, \beta_2, \beta_3, \ldots\ldots$, in succession.

The logarithms of $m_1, m_2, \ldots\ldots$, as far as it has been necessary to use them in the construction of Table II, are given at the end of this paper in Table VI.

14. In going through the calculation, it is, of course, useless to take the values of the quantities $\beta_1, \beta_2, \ldots\ldots$, to a decimal place further than the last one which can be accurately obtained in β_0. If ten-figure logarithms be used, ten significant figures can be ordinarily obtained with accuracy from the logarithm. Of this the writer has satisfied himself by working out the value of $(\pi/2x)^{\frac{1}{2}}\epsilon^{-x}$ by elementary arithmetic and the exponential theorem, for one or two simple values of x, as $x = 8$, $x = 11$, and comparing the result so obtained with that derived from the logarithms. They always agree for ten places, sometimes for eleven, if account be taken of the second differences of the logarithms.

It follows that for larger values of x, for which the smallest term in the series is less than $10^{-10}\beta_0$, the value of $K_0(x)$ can be obtained with accuracy, probably for ten, and pretty certainly for nine significant figures. The tenth figure may be in error owing to the accumulation, in addition, of the errors in the last places of the quantities $\beta_1, \beta_2, \ldots\ldots$.

15. Equation (13) gives

$$K_1(x) = -\left(\frac{\pi}{2x}\right)^{\frac{1}{2}}\epsilon^{-x}\left\{1+\frac{3}{8x}-\frac{3\cdot 5}{1\cdot 2(8x)^2}+\ldots\ldots\right\}.$$

The multipliers, disregarding sign, by which the coefficients of the successive powers of x within the bracket are derived, each from the preceding, are

$$\tfrac{3}{8}, \tfrac{5}{16}, \tfrac{21}{24}, \tfrac{45}{32}, \ldots\ldots$$

Let these be denoted by the symbols $\mu_1, \mu_2, \mu_3, \ldots\ldots$, and let a series of quantities $\beta'_1, \beta'_2, \beta'_3, \ldots\ldots$, be obtained from β_0 by the successive relations

$$\beta'_1 = \mu_1\beta_0 x^{-1}, \quad \beta'_2 = \mu_2\beta'_1 x^{-1}, \quad \beta'_3 = \mu_3\beta_2 x^{-1}, \ldots\ldots \quad (16)$$

β_0 having the same value as in Article (13).

Then evidently

$$\begin{aligned} K_1(x) &= -\{\beta_0+\beta'_1-\beta'_2+\beta'_3-\beta'_4+\ldots\ldots\} \\ &= -[(\beta_0+\beta'_1+\beta'_3+\ldots\ldots)-(\beta'_2+\beta'_4+\ldots\ldots)]; \end{aligned}$$

the summations being carried on, either until the smallest term of the series is reached, in the case of the lower values of x, or until a term is arrived at which is less than $10^{-10}\beta_0$, which will happen first for larger values of x.

The relations (16) are adapted to logarithmic computation. The logarithms of μ_1, μ_2, μ_3, ... are given in Table VI.

16. The verification of the values of $K_0(x)$, $K_1(x)$ in Table II, cannot be conducted on the method applied to those in Table I, because the values of $I_0(x)$, $I_1(x)$ are wanting.

A certain amount of check is given by the values of the four functions I_0, I_1, K_0, K_1, calculated for the integral values of x, by the former method, given in Table III.

Two other checks, in addition to the useful one of performing all additions and multiplications in two different ways, have been applied throughout.

The first depends on a very simple relation between the quantities β_r and β'_r.

It is easily seen, from the general formula for the $\overline{r+1}$th term in (9), that

$$\frac{\beta'_r}{\beta_r} = \frac{3\cdot 5\cdot 21 \ldots\ldots \{(2r-1)^2-4\}}{1\cdot 3^2\cdot 5^2 \ldots\ldots \quad (2r-1)^2};$$

which, since $(2r-1)^2-4=(2r+1)(2r-3)$, easily reduces to

$$(2r+1)/(2r-1).$$

Thus $$\beta'_r = \frac{2r+1}{2r-1}\beta_r = \beta_r\left(1+\frac{2}{2r-1}\right) \qquad (17)$$

When the quantities β and β' have been calculated from the logarithmic formulæ, this result gives an easy method of verification. It detects any mistake in the computation of the logarithms, or in the derivation of the number from the logarithm.

This formula leaves untouched the possibility of a mistake in the value of β_0. To check this another process has been used.

17. If $f(x)$ be any continuous function of x, whose differential coefficients are also finite and continuous for the values of x considered, Taylor's Theorem gives

$$f(x+h) = f(x)+hf^{\text{I}}(x)+\frac{h^2}{\Pi(2)}f^{\text{II}}(x)+\ldots\ldots$$

Let u_0 be the value of $f(x)$ corresponding to any particular value x_0 of x, and let u_1, u_2, u_3, ... denote the values of $f(x_0+h)$, $f(x_0+2h)$, $f(x_0+3h)$, ... Similarly, let u_{-1}, u_{-2}, u_{-3} ... denote $f(x_0-h)$, $f(x_0-2h)$, $f(x_0-3h)$, ...

Then $$\frac{u_1-u_{-1}}{2h} = f^{\text{I}}(x_0)+\frac{h^2}{6}f^{\text{III}}(x_0)+\frac{h^4}{120}f^{\text{V}}(x_0)+\ldots\ldots$$

If h be so small that the terms of the series on the right hand not written down may be neglected, and the three terms written down be denoted by u, v, w, respectively, it follows that

$$u+v+w = \frac{u_1-u_{-1}}{2h} = \alpha \text{ say};$$

writing $2h$ for h, this gives

$$u+4v+16w = \frac{u_2-u_{-2}}{4h} = \beta,$$

and putting $3h$ for h

$$u+9v+81w = \frac{u_3-u_{-3}}{6h} = \gamma.$$

From these three equations u can be found in terms of α, β, γ, and its value can be put into the convenient shape

$$u = \alpha - \tfrac{1}{2}(\beta-\alpha) + \tfrac{1}{10}(\gamma-\beta) \quad\text{.....................(18)}$$

From the sequence law it follows that

$$K_1 = dK_0/dx.$$

Consequently, if values of $K_0(x)$ for seven equidistant values of x be taken, the quantities α, β, and γ can be derived, and (18) ought to give a value of u equal to that of $K_1(x)$ for the middle value of x. This test has been freely applied throughout Table II with very satisfactory results.

18. If the values of $f(x_0+4h)$ and $f(x_0-4h)$ be taken into account, a still more stringent test is afforded. As before, let these quantities be denoted by u_4 and u_{-4}. Let z be used to denote

$$\frac{h^6}{\Pi(7)} f^{\text{vii}}(x_0) \text{ and let } \frac{u_4-u_{-4}}{8h} = \delta.$$

Then

$$\begin{aligned} u+v+w+z &= \alpha, \\ u+4v+16w+256z &= \beta, \\ u+9v+81w+729z &= \gamma, \\ u+16v+256w+4096z &= \delta; \end{aligned}$$

whence it is not difficult to show that

$$u = \alpha - \tfrac{3}{5}(\beta-\alpha) + \frac{\gamma-\beta}{5} - \frac{\delta-\gamma}{35} \quad\text{.................. (19)}$$

This can be used independently, or it can be made to yield a correction to (18). In the latter case the quantity

$$\tfrac{1}{10}(\beta-\alpha) + \tfrac{1}{35}(\delta-\gamma) - \tfrac{1}{10}(\gamma-\beta) \quad\text{.............. (20)}$$

has to be subtracted from the value of u given by (18).

19. As an example, suppose the value of x_0 is taken as 7·4. Table II gives

$$\begin{aligned} u_{-4} &= 0{\cdot}000\ 424\ 795\ 741 \\ u_{-3} &= 0{\cdot}000\ 381\ 739\ 385 \\ u_{-2} &= 0{\cdot}000\ 343\ 079\ 156 \\ u_{-1} &= 0{\cdot}000\ 308\ 362\ 213 \\ u_{1} &= 0{\cdot}000\ 249\ 177\ 616 \\ u_{2} &= 0{\cdot}000\ 224\ 020\ 677 \\ u_{3} &= 0{\cdot}000\ 201\ 420\ 050 \\ u_{4} &= 0{\cdot}000\ 181\ 113\ 953 \end{aligned}$$

whence, remembering that $h = \frac{1}{10}$, it easily follows that, a minus sign being understood before all the numbers,

$$\begin{aligned} \alpha &= 0{\cdot}000\ 295\ 922\ 985 \\ \beta &= 0{\cdot}000\ 297\ 646\ 197 \\ \gamma &= 0{\cdot}000\ 300\ 532\ 225 \\ \delta &= 0{\cdot}000\ 304\ 602\ 235 \end{aligned}$$

whence

$$\begin{aligned} \beta-\alpha &= 0{\cdot}000\ 001\ 723\ 212 \\ \gamma-\beta &= 0{\cdot}000\ 002\ 886\ 028 \\ \delta-\gamma &= 0{\cdot}000\ 004\ 070\ 010 \end{aligned}$$

Hence, using the formula (18),

$$\begin{aligned} \alpha &= 0{\cdot}000\ 295\ 922\ 985 \\ \tfrac{1}{10}(\gamma-\beta) &= 0{\cdot}000\ 000\ 288\ 603 \\ \hline & \ 0{\cdot}000\ 296\ 211\ 588 \\ \tfrac{1}{2}(\beta-\alpha) &= 0{\cdot}000\ 000\ 861\ 606 \\ \hline & \ 0{\cdot}000\ 295\ 349\ 982 \end{aligned}$$

The value in the table for K_1 (7·4) is 0·000 295 349 978.

If we apply the correction (20), the above values give

$$\begin{aligned} \tfrac{1}{10}(\beta-\alpha) &= 000\ 000\ 172\ 321 \\ \tfrac{1}{33}(\delta-\gamma) &= \qquad\quad\ 116\ 286 \\ \hline & \qquad\quad\ 288\ 607 \\ \tfrac{1}{10}(\gamma-\beta) &= \qquad\quad\ 288\ 603 \\ \hline \text{Correction} &= 000\ 000\ 000\ 004 \end{aligned}$$

This has to be subtracted from the former value, and the result agrees exactly with the value of $K_1(7{\cdot}4)$ in Table II.

The agreement is not in all cases quite so exact as in this example, as may be expected from the necessary existence of more or less of error *in the* last figures taken into account.

A slight additional verification of the general accuracy of Table II has been gained by the calculation of the term β_0 for the values 8, 9, 10, 11, and 12 by elementary arithmetic and the exponential theorem without the use of logarithms.

The last figure of the quantities in Table II cannot be depended on for strict accuracy, in which respect the table differs from Table I.

20. A farther extension of the formulæ of Articles 17 and 18 has some interest.

If, with the same notation extended, the quantity $(u_5 - u_{-5})/10h$ be denoted by ϵ, it is not difficult to prove that

$$u = \alpha - \tfrac{2}{3}(\beta - \alpha) + \tfrac{2}{7}(\gamma - \beta) - \frac{\delta - \gamma}{14} + \frac{\epsilon - \delta}{126} \quad \cdots\cdots\cdots \quad (21).$$

This value of u can most easily be computed by subtracting

$$\tfrac{1}{6}(\beta - \alpha) + \tfrac{1}{70}(\gamma - \beta) + \tfrac{1}{14}(\delta - \gamma) - \tfrac{1}{6}(\gamma - \beta) - \frac{\epsilon - \delta}{126}$$

from the value of u given in (18).

This farther correction is too small to be applied with any certainty to the values of $K_1(x)$ derived from $K_0(x)$ in Table II. Obviously however, all these formulæ may be equally well applied to Table I, and throughout the range of that table, this formula deduces a value of $K_1(x)$ more accurate to one or two places than that given in (19).

To give two examples; one from the earlier part of the table.

If $x = 2{\cdot}6$

Equation (18) gives	$-K_1(x)$	=	0·065 284 052 521 550
„ (19) „	$-K_1(x)$	=	0·065 284 044 927 362
„ (21) „	$-K_1(x)$	=	0·065 284 045 062 511
while the correct value is			0·065 284 045 058 531

Again taking the largest value of x in Table I which admits of the application of (21), namely $x = 5{\cdot}5$,

Equation (18) gives	$-K_1(x)$	=	0·002 325 569 051 888
„ (19) „	$-K_1(x)$	=	0·002 325 569 008 660
„ (21) „	$-K_1(x)$	=	0·002 325 569 008 850
while the correct value is			0·002 325 569 008 849 005

None of these formulæ is sufficient for verification of the values in Table I to the last figure given.

Table I.

x.	$I_0(x)$.	$I_1(x)$.
0·1	1·002 501 562 934 095 601 400	0·050 062 526 047 092 692 114·
0·2	1·010 025 027 795 145 835 263·	0·100 500 834 028 125 115 768
0·3	1·022 626 879 351 596 991 120·	0·151 693 840 003 592 780 329·
0·4	1·040 401 782 229 341 241 022·	0·204 026 755 733 570 596 281
0·5	1·063 483 370 741 323 519 263	0·257 894 305 390 896 316 362
0·6	1·092 045 364 317 339 541 841·	0·313 704 025 604 922 130 966
0·7	1·126 303 018 306 809 198 051·	0·371 879 677 777 008 654 743·
0·8	1·166 514 922 869 802 731 431	0·432 864 802 620 639 821 166·
0·9	1·212 985 165 728 684 317 724·	0·497 126 448 160 964 276 677
1·0	1·266 065 877 752 008 335 598	0·565 159 103 992 485 027 208·
1·1	1·326 160 183 712 652 485 589	0·637 488 876 453 881 892 572·
1·2	1·393 725 584 134 064 395 588	0·714 677 941 552 643 086 231
1·3	1·469 277 797 944 250 888 664	0·797 329 314 979 268 902 964
1·4	1·553 395 099 731 216 509 982·	0·886 091 981 414 327 353 583·
1·5	1·646 723 189 772 890 844 876	0·981 666 428 577 907 585 652
1·6	1·749 980 639 738 909 390 905	1·084 810 635 129 879 617 220·
1·7	1·863 964 962 073 839 671 192	1·196 346 565 634 482 268 430·
1·8	1·989 559 356 618 050 914 345	1·317 167 230 391 898 987 579·
1·9	2·127 740 194 053 887 856 891	1·448 244 373 054 888 953 884·
2·0	2·279 585 302 336 067 267 437	1·590 636 854 637 329 063 382
2·1	2·446 283 129 436 182 291 275	1·745 499 808 836 106 159 137
2·2	2·629 142 863 567 314 172 737·	1·914 094 650 586 386 159 283
2·3	2·829 605 600 627 585 665 907	2·097 800 027 517 421 476 844·
2·4	3·049 256 657 989 418 844 196·	2·298 123 812 543 222 324 570
2·5	3·289 839 144 050 123 035 706·	2·516 716 245 288 698 441 528
2·6	3·553 268 904 243 671 659 925·	2·755 384 340 504 706 456 568
2·7	3·841 650 976 595 934 202 977	3·016 107 693 161 405 855 985
2·8	4·157 297 703 500 820 202 310	3·301 055 822 635 087 581 928
2·9	4·502 748 661 326 274 366 311·	3·612 607 212 436 907 736 703·
3·0	4·880 792 585 865 024 085 611	3·953 370 217 402 609 396 479·
3·1	5·294 491 489 675 606 473 324·	4·326 206 027 313 598 387 154
3·2	5·747 207 187 180 549 677 026·	4·734 253 894 709 620 419 983·
3·3	6·242 630 465 183 028 963 790	5·180 958 855 355 928 605 292
3·4	6·784 813 160 431 586 596 268·	5·670 102 192 635 219 559 794
3·5	7·378 203 432 225 479 660 344·	6·205 834 922 258 365 473 623·
3·6	8·027 684 547 054 009 945 933	6·792 714 601 361 299 242 400
3·7	8·738 617 524 169 395 584 970	7·435 745 796 535 335 730 518·
3·8	9·516 888 026 098 957 047 398	8·140 424 578 907 955 806 110
3·9	10·368 957 916 732 943 985 764	8·912 787 451 362 725 689 348·
4·0	11·301 921 952 136 330 496 356	9·759 465 153 704 449 909 475
4·1	12·323 570 116 019 571 436 934·	10·687 741 836 417 761 231 468·
4·2	13·442 456 163 297 646 200 379	11·705 620 143 051 615 977 998·
4·3	14·667 972 991 845 562 465 006	12·821 892 795 648 573 301 862
4·4	16·010 435 524 946 996 723 558·	14·046 221 337 533 105 734 577·
4·5	17·481 171 855 609 276 043 133	15·389 222 753 735 923 892 694·
4·6	19·092 623 479 519 459 002 267	16·862 564 761 076 656 391 871
4·7	20·858 455 526 644 462 400 770·	18·479 070 647 133 100 245 291
4·8	22·793 677 993 105 797 960 124	20·252 834 600 238 559 989 488·
4·9	24·914 779 075 887 756 060 699	22·199 348 620 092 491 190 354·
5·0	27·239 871 823 604 446 894 544	24·335 642 142 450 527 199 143
5·1	29·788 855 440 238 848 499 153	26·680 435 679 477 119 089 197
5·2	32·583 592 710 613 699 532 308	29·254 309 881 798 348 760 365
5·3	35·648 105 168 113 101 763 145	32·079 891 578 297 025 753 268
5·4	39·008 787 785 625 836 242 827	35·182 058 506 083 583 786 328·
5·5	42·694 645 151 847 784 559 282	38·588 164 616 327 393 255 945·
5·6	46·737 551 292 637 286 856 629	42·328 288 032 466 848 420 202·
5·7	51·172 535 515 159 998 128 205	46·435 503 947 521 351 864 819
5·8	56·038 096 892 622 866 750 874	50·946 184 978 774 806 273 857
5·9	61·376 550 271 771 251 908 395	55·900 331 753 160 078 871 856·
6·0	67·234 406 976 477 975 326 188	61·341 936 777 640 237 861 329

Table I.

$K_0(x)$.	$-K_1(x)$.	x.
2·427 069 024 702 016 612 519·	9·853 844 780 870 606 134 849·	0·1
1·752 703 855 528 145 906 617	4·775 972 543 220 472 248 750·	0·2
1·372 460 060 544 297 376 645·	3·055 992 033 457 324 978 851·	0·3
1·114 529 134 524 434 406 170·	2·184 354 424 732 687 379 723	0·4
0·924 419 071 227 665 861 782·	1·656 441 120 003 300 893 696	0·5
0·777 522 091 904 729 289 468·	1·302 834 939 763 502 176 671	0·6
0·660 519 859 915 101 548 740	1·050 283 535 312 917 951 430	0·7
0·565 347 105 265 895 668 369	0·861 781 634 472 180 346 690·	0·8
0·486 730 308 162 900 521 582·	0·716 533 578 776 019 074 786	0·9
0·421 024 438 240 708 333 336·	0·601 907 230 197 234 574 738·	1·0
0·365 602 391 543 185 880 566·	0·509 760 027 167 027 048 822·	1·1
0·318 508 220 286 593 615 118·	0·434 592 391 060 715 038 502·	1·2
0·278 247 646 300 026 999 011	0·372 547 495 631 962 166 173·	1·3
0·243 655 061 181 541 893 927·	0·320 835 902 229 875 750 946·	1·4
0·213 805 562 647 525 736 722·	0·277 387 800 456 843 816 085	1·5
0·187 954 751 969 332 325 059	0·240 633 911 357 611 855 164·	1·6
0·165 496 318 056 996 539 364·	0·209 362 488 204 082 474 675·	1·7
0·145 931 400 489 827 981 234	0·182 623 099 801 746 979 604·	1·8
0·128 845 979 276 047 479 856·	0·159 660 153 032 667 610 382	1·9
0·113 893 872 749 533 435 653·	0·139 865 881 816 522 427 285·	2·0
0·100 783 740 889 966 945 812·	0·122 746 411 533 507 910 608·	2·1
0·089 269 005 671 601 745 130·	0·107 896 810 119 087 275 030·	2·2
0·079 139 933 002 093 626 828	0·094 982 443 845 362 636 833	2·3
0·070 217 341 543 415 895 531	0·083 724 838 754 832 182 453·	2·4
0·062 347 553 200 366 186 029	0·073 890 816 347 747 063 649·	2·5
0·055 398 303 286 321 951 484	0·065 284 045 058 531 495 000	2·6
0·049 255 400 915 817 592 455	0·057 738 398 956 525 947 419·	2·7
0·043 819 981 975 498 528 903	0·051 112 685 607 272 438 995	2·8
0·039 006 234 566 223 424 101·	0·045 286 423 298 361 443 561	2·9
0·034 739 504 386 279 248 072	0·040 156 431 128 194 184 377·	3·0
0·030 954 708 038 041 442 502·	0·035 634 054 949 617 493 670	3·1
0·027 594 997 675 100 610 315	0·031 642 895 211 398 770 897	3·2
0·024 610 632 145 839 314 335·	0·028 116 934 272 716 612 255·	3·3
0·021 958 018 806 808 280 394·	0·024 998 984 123 186 272 784	3·4
0·019 598 897 170 368 489 108·	0·022 239 392 925 923 833 739·	3·5
0·017 499 641 018 146 603 343	0·019 794 962 019 720 617 134·	3·6
0·015 630 659 921 626 661 612	0·017 628 035 102 223 266 688·	3·7
0·013 965 884 534 245 617 659·	0·015 705 729 078 473 492 808·	3·8
0·012 482 322 757 249 775 684	0·013 999 282 082 274 828 044·	3·9
0·011 159 676 085 853 024 270·	0·012 483 498 887 268 431 470	4·0
0·009 980 007 227 840 242 646·	0·011 136 277 633 479 931 554	4·1
0·008 927 451 541 542 371 598	0·009 938 204 735 917 087 547·	4·2
0·007 987 966 031 764 522 372	0·008 872 207 188 591 397 612·	4·3
0·007 149 110 623 307 253 932·	0·007 923 253 361 445 598 749·	4·4
0·006 399 857 243 233 975 046·	0·007 078 094 908 968 089 693·	4·5
0·005 730 422 917 292 834 887	0·006 325 043 644 264 015 020·	4·6
0·005 132 123 648 454 615 086·	0·005 653 778 240 030 826 704	4·7
0·004 597 246 316 724 657 899	0·005 055 176 444 056 299 816·	4·8
0·004 118 936 235 515 888 790·	0·004 521 169 177 299 838 509	4·9
0·003 691 098 334 042 594 275·	0·004 044 613 445 452 164 208	5·0
0·003 308 310 218 017 464 327·	0·003 619 181 462 317 798 328·	5·1
0·002 965 745 601 029 581 462	0·003 239 263 773 089 456 376	5·2
0·002 659 106 803 389 557 342·	0·002 899 884 491 690 688 906	5·3
0·002 384 565 189 724 900 197	0·002 596 627 040 177 797 776·	5·4
0·002 138 708 565 950 287 432·	0·002 325 569 008 849 005 155	5·5
0·001 918 494 684 356 577 228	0·002 083 224 950 609 789 166	5·6
0·001 721 210 115 723 315 288	0·001 866 496 088 311 830 924	5·7
0·001 544 433 842 281 102 204·	0·001 672 626 054 141 651 512	5·8
0·001 386 005 007 804 947 106·	0·001 499 161 899 722 485 306·	5·9
0·001 243 994 328 013 123 085	0·001 343 919 717 785 509 008·	6·0

Table II.

x.	$K_0(x)$.	$-K_1(x)$.	x.	x.	$K_0(x)$.	$-K_1(x)$.	x.
5·0	0·003 691 098	0·004 044 614	**5·0**	**8·5**	0·000 086 257 566 3	0·000 091 197 247 7	**8·5**
5·1	0·003 308 310	0·003 619 182	5·1	8·6	0·000 077 605 920 7	0·000 081 999 731 8	8·6
5·2	0·002 965 746	0·003 239 264	5·2	8·7	0·000 069 826 521 36	0·000 073 735 540 60	8·7
5·3	0·002 659 107	0·002 899 884	5·3	8·8	0·000 062 830 892 86	0·000 066 809 267 33	8·8
5·4	0·002 384 565	0·002 596 627	5·4	8·9	0·000 056 539 599 34	0·000 059 635 344 08	8·9
5·5	0·002 138 709	0·002 325 569	**5·5**	**9·0**	0·000 050 881 312 956	0·000 053 637 016 382	**9·0**
5·6	0·001 918 495	0·002 083 225	5·6	9·1	0·000 045 791 979 331	0·000 048 245 426 023	9·1
5·7	0·001 721 210	0·001 866 496	5·7	9·2	0·000 041 214 069 631	0·000 043 398 790 454	9·2
5·8	0·001 544 433 7	0·001 672 626 1	5·8	9·3	0·000 037 095 910 423	0·000 039 041 668 525	9·3
5·9	0·001 386 005 0	0·001 499 161 9	5·9	9·4	0·000 033 391 083 017	0·000 035 124 303 368	9·4
6·0	0·001 243 994 8	0·001 343 919 7	**6·0**	**9·5**	0·000 030 057 884 958	0·000 031 602 034 110	**9·5**
6·1	0·001 116 678 7	0·001 204 954 3	6·1	9·6	0·000 027 058 847 266	0·000 028 434 769 224	9·6
6·2	0·001 002 518 9	0·001 080 532 4	6·2	9·7	0·000 024 360 301 507	0·000 025 586 514 844	9·7
6·3	0·000 900 139 2	0·000 969 108 8	6·3	9·8	0·000 021 931 991 556	0·000 023 024 952 359	9·8
6·4	0·000 808 809 9	0·000 869 305 8	6·4	9·9	0·000 019 746 725 314	0·000 020 721 059 930	9·9
6·5	0·000 725 931 8	0·000 779 894 4	**6·5**	**10·0**	0·000 017 780 062 816	0·000 018 648 773 453 9	**10·0**
6·6	0·000 652 021 37	0·000 699 777 68	6·6	10·1	0·000 016 010 033 412	0·000 016 784 682 675	10·1
6·7	0·000 585 699 16	0·000 627 976 68	6·7	10·2	0·000 014 416 889 253	0·000 015 107 758 866	10·2
6·8	0·000 526 178 09	0·000 563 617 16	6·8	10·3	0·000 012 982 874 576	0·000 013 599 110 702	10·3
6·9	0·000 472 753 79	0·000 505 918 31	6·9	10·4	0·000 011 692 025 596	0·000 012 241 765 367	10·4
7·0	0·000 424 795 74	0·000 454 182 49	**7·0**	**10·5**	0·000 010 529 988 143	0·000 011 020 472 310	**10·5**
7·1	0·000 381 739 385	0·000 407 786 222	7·1	10·6	0·000 009 483 854 408	0·000 009 921 527 234	10·6
7·2	0·000 343 079 156	0·000 366 172 174	7·2	10·7	0·000 008 542 016 344 7	0·000 008 932 614 226	10·7
7·3	0·000 308 362 213	0·000 328 841 997	7·3	10·8	0·000 007 694 034 041 2	0·000 008 042 664 131 7	10·8
7·4	0·000 277 182 870	0·000 295 349 978	7·4	10·9	0·000 006 930 517 517 5	0·000 007 241 727 528 8	10·9
7·5	0·000 249 177 617	0·000 265 297 890	**7·5**	**11·0**	0·000 006 243 020 547 6	0·000 006 520 860 674 6	**11·0**
7·6	0·000 221 020 678	0·000 238 327 458	7·6	11·1	0·000 005 623 945 302 6	0·000 005 872 023 261 0	11·1
7·7	0·000 201 420 050	0·000 214 120 873	7·7	11·2	0·000 005 066 456 681 9	0·000 005 287 986 539 5	11·2
7·8	0·000 181 113 953	0·000 192 391 797	7·8	11·3	0·000 004 564 405 350 1	0·000 004 762 250 929 6	11·3
7·9	0·000 162 867 668	0·000 172 884 307	7·9	11·4	0·000 004 112 258 592 2	0·000 004 288 972 021 7	11·4
8·0	0·000 146 470 705 2	0·000 155 369 211 8	**8·0**	**11·5**	0·000 003 705 038 165 4	0·000 003 862 894 145 8	**11·5**
8·1	0·000 131 734 278 6	0·000 139 641 228 9	8·1	11·6	0·000 003 338 264 475 1	0·000 003 479 290 732 4	11·6
8·2	0·000 118 489 040 5	0·000 125 516 451 2	8·2	11·7	0·000 003 007 906 380 0	0·000 003 133 910 741 2	11·7
8·3	0·000 106 583 050 1	0·000 112 830 094 0	8·3	11·8	0·000 002 710 336 093 0	0·000 002 822 930 559 3	11·8
8·4	0·000 095 880 013 8	0·000 101 434 481 3	8·4	11·9	0·000 002 442 288 637 0	0·000 002 542 910 795 2	11·9
8·5	0·000 086 257 566 3	0·000 091 197 247 7	**8·5**	**12·0**	0·000 002 200 825 397 302	0·000 002 290 757 464 767	**12·0**

Table III.

x.	$I_0(x)$.	$I_1(x)$.	$K_0(x)$.	$-K_1(x)$.	x.
6·0	67·234 406 976 477 975 326	61·341 936 777 640 237 861	0·001 243 994 328 018 123	0·001 343 919 717 735 509	6·0
7·0	168·593 908 510 289 698 857	156·039 092 869 955 453 462	0·000 424 795 741 869 231	0·000 454 182 486 884 898	7·0
8·0	427·564 115 721 804 785 175	399·873 136 782 560 098 228	0·000 146 470 705 222 804	0·000 155 369 211 804 984	8·0
9·0	1093·583 354 511 374 695 845	1030·914 722 516 956 444 428	0·000 050 881 312 956 458	0·000 053 637 016 379 453	9·0
10·0	2815·716 628 466 254 471 294	2670 988 303 701 254 654 247	0·000 017 780 062 316 066	0·000 018 648 773 453 874	10·0
11·0	7288·489 339 821 248 106 179	6948·858 659 812 163 230 818	0·000 006 243 020 547 653	0·000 006 520 860 674 582	11·0

8 2

Table IV.

x.	$\text{Log}\, x - \text{E} - 1$.	x.
0·1	3·418 516 608 652 458 132 828 711 486 060	0·1
0·2	2·725 369 428 092 512 823 411 479 364 602	0·2
0·3	2·319 904 319 984 348 441 433 466 249 138	0·3
0·4	2·032 222 247 532 567 513 994 247 243 144	0·4
0·5	1·809 078 696 218 357 758 227 952 152 834	0·5
0·6	1·626 757 139 424 403 132 016 234 127 680	0·6
0·7	1·472 606 459 597 144 827 723 358 742 617	0·7
0·8	1·339 075 066 972 622 204 577 015 121 686	0·8
0·9	1·221 292 031 316 238 750 038 221 012 216	0·9
1·0	1·115 931 515 658 412 448 810 720 031 376	1·0
1·1	1·020 621 335 854 087 588 766 767 908 095	1·1
1·2	0·933 609 958 864 457 822 599 002 006 222	1·2
1·3	0·853 567 251 190 921 396 775 224 044 495	1·3
1·4	0·779 459 279 037 199 518 306 126 621 159	1·4
1·5	0·710 466 407 550 248 066 832 706 915 912	1·5
1·6	0·645 927 886 412 676 895 159 783 000 228	1·6
1·7	0·585 303 264 596 242 052 579 176 868 188	1·7
1·8	0·528 144 850 756 293 440 620 988 890 758	1·8
1·9	0·474 077 629 486 017 672 819 684 054 173	1·9
2·0	0·422 784 335 098 467 139 393 487 909 918	2·0
2·1	0·373 994 170 929 035 136 328 113 505 694	2·1
2·2	0·327 474 155 294 142 279 349 535 786 637	2·2
2·3	0·283 022 392 723 308 442 021 958 654 250	2·3
2·4	0·240 462 778 304 512 513 181 769 884 763	2·4
2·5	0·199 640 783 784 257 383 627 192 819 608	2·5
2·6	0·160 420 070 630 976 087 357 991 923 037	2·6
2·7	0·122 679 742 648 129 058 642 975 775 293	2·7
2·8	0·086 312 098 477 254 208 888 894 499 701	2·8
2·9	0·051 220 778 665 984 105 645 439 463 698	2·9
3·0	0·017 319 226 990 302 757 415 474 794 454	3·0
3·1	0·015 470 595 832 688 113 100 452 838 482	3·1
3·2	0·047 219 294 147 268 414 257 449 121 230	3·2
3·3	0·077 990 952 814 022 102 628 477 328 827	3·3
3·4	0·107 843 915 963 703 256 838 055 253 271	3·4
3·5	0·136 831 452 836 955 546 877 400 590 609	3·5
3·6	0·165 002 329 803 651 868 796 243 230 701	3·6
3·7	0·192 401 303 991 766 311 539 384 184 971	3·7
3·8	0·219 069 551 073 927 636 597 548 067 286	3·8
3·9	0·245 045 037 477 188 294 620 021 192 428	3·9
4·0	0·270 362 845 461 478 170 023 744 211 540	4·0
4·1	0·295 055 458 051 849 671 038 051 886 977	4·1
4·2	0·319 153 009 630 910 173 089 118 615 764	4·2
4·3	0·342 683 507 041 104 290 644 131 027 286	4·3
4·4	0·365 673 025 265 803 030 067 696 334 821	4·4
4·5	0·388 145 881 117 861 624 562 538 321 011	4·5
4·6	0·410 124 787 836 636 867 395 273 467 208	4·6
4·7	0·431 630 993 057 600 453 992 239 183 712	4·7
4·8	0·452 684 402 255 432 796 235 462 236 695	4·8
4·9	0·473 303 689 458 168 477 381 994 000 826	4·9
5·0	0·493 506 396 775 687 925 790 039 301 850	5·0
5·1	0·513 309 024 071 867 638 816 068 368 736	5·1
5·2	0·532 727 109 928 969 222 059 240 198 422	5·2
5·3	0·551 775 304 899 663 701 315 757 652 969	5·3
5·4	0·570 467 437 911 816 250 774 256 346 166	5·4
5·5	0·588 816 576 580 012 785 833 991 425 131	5·5
5·6	0·606 835 082 082 691 100 528 337 621 758	5·6
5·7	0·624 534 659 182 092 018 575 561 182 750	5·7
5·8	0·641 926 401 893 961 208 771 792 667 760	5·8
5·9	0·659 020 835 253 261 317 787 388 887 660	5·9
6·0	0·675 827 953 569 642 552 001 757 327 004	6·0

Old numeral type (518) in Tables IV and VI denotes negative quantities.

Table V.

n.	$S_n - 1$.
6	1·450
7	1·592 857 142 857 142
8	1·717 857 142 857 142
9	1·828 968 253 968 253
10	1·928 968 253 968 253
11	2·019 877 344 877 344
12	2·103 210 678 210 678
13	2·180 133 755 133 755
14	2·251 562 326 562 326
15	2·318 228 993 228 993
16	2·380 728 993 228 993 228
17	2·439 552 522 639 757 934 875 580
18	2·495 108 078 195 313 490 431 135
19	2·547 739 657 142 681 911 483 766
20	2·597 739 657 142 681 911 483 766
21	2·645 358 704 761 729 530 531 385
22	2·690 813 250 216 274 985 076 839
23	2·734 291 511 065 840 202 468 143
24	2·775 958 177 752 506 869 134 809
25	2·815 958 177 752 506 869 134 809
26	2·854 419 716 314 045 326
27	2·891 456 753 351 082 363
28	2·927 171 039 065 368 077

Table VI.

n.	Log m_n.	Log μ_n.	n.
1	$\bar{1}$·096 9100 130	$\bar{1}$·574 0312 677	1
2	$\bar{1}$·750 1225 267	$\bar{1}$·494 8500 216	2
3	0·017 7287 670	$\bar{1}$·942 0080 530	3
4	0·185 0461 017	0·148 0625 355	4
5	0·306 4250 276	0·284 4307 339	5
6	0·401 5441 329	0·386 9446 243	6
7	0·479 6986 776	0·469 2959 172	7
8	0·546 0025 441	0·538 2122 997	8
9	0·603 5653 464	0·597 5123 636	9
10	0·654 4172 149	0·649 5782 291	10
11	0·699 9559 173	0·695 9987 648	11
12	0·741 1844 390	0·737 8880 704	12
13	0·778 8466 780	0·776 0582 609	13
14	0·813 5095 056	0·811 1199 839	14
15	0·845 6147 498	0·843 5442 120	15
16	0·875 5134 181	0·873 7019 682	16
17	0·903 4889 714	0·901 8908 298	17
18	0·929 7735 966	0·928 3531 718	18
19	0·954 5598 602	0·953 2890 635	19
20	0·978 0092 314	0·976 8655 981	20
21	1·000 2584 317	0·999 2237 809	21
22	1·021 4242 434	1·020 4837 027	22
23	1·041 6072 046	1·040 7484 905	23
24	1·060 8944 872	1·060 1073 651	24
25	1·079 3621 644	1·078 6380 383	25

"Memoir on the Theory of the Partitions of Numbers. Part II." By Major P. A. MACMAHON, R.A., D.Sc., F.R.S. Received November 21,—Read November 24, 1898.

(Abstract.)

Introduction.

The subject of the partition of numbers, for its proper development, requires treatment in a new and more comprehensive manner. The subject matter of the theory needs enlargement. This will be found to be a necessary consequence of the new method of regarding a partition that is here brought into prominence.

Let an integer n be broken up into any number of integers

$$\alpha_1,\ \alpha_2,\ \alpha_3,\ldots\ldots\alpha_s,$$

and we ascribe the conditions

$$\alpha_1 \geqq \alpha_2 \geqq \alpha_3 - \ldots\ldots \quad \alpha_s,$$

the succession

$$\alpha_1,\ \alpha_2,\ \alpha_3\ldots\ldots\alpha_s$$

is what is known as a partition of n.

There are $s-1$ conditions

$$\alpha_1 \geqq \alpha_2,\ \alpha_2 \geqq \alpha_3\ldots\ldots\alpha_{s-1} \geqq \alpha_s,$$

to which we may add

$$\alpha_s \geqq 0,$$

if the integers be all of them positive (or zero).

For the present all the integers are restricted to the positive or zero by hypothesis, so that this last-written condition will not be further attended to.

If, on the other hand, the conditions be

$$\alpha_1 \gtreqless \alpha_2 \gtreqless \alpha_3\ldots\ldots \gtreqless \alpha_s$$

no order of magnitude is supposed to exist between the successive parts, and we obtain what has been termed a "composition" of the integer n.

Various other systems of partitions into s parts may be brought under view, because between two consecutive parts we may place either of the seven symbols

$$>,\ =,\ <,\ \geqq,\ \leqq,\ \neq,\ \gtreqless.$$

We thus obtain 7^{s-1} different sets of conditions that may be assigned; these are not all essentially different, and in many cases they overlap.

For the moment I concentrate attention upon the symbol

$$\geqq$$

and remark that the $s-1$ conditions which involve this symbol, set forth above, constitute one set of a larger class of sets which involve the symbol. We may have the single condition

$$A_1^{(1)}\alpha_1 + A_2^{(1)}\alpha_2 + A_3^{(1)}\alpha_3 + \ldots\ldots + A_s^{(1)}\alpha_s \geqq 0,$$

wherein A_1, A_2, $A_3 \ldots\ldots A_s$ are integers $+$, zero, or $-$, of which at least one must be positive, or we may have the set of conditions

$$\left.\begin{array}{l} A_1^{(1)}\alpha_1 + A_2^{(1)}\alpha_2 + A_3^{(1)}\alpha_3 + \ldots\ldots + A_s^{(1)}\alpha_s \geqq 0 \\ A_1^{(2)}\alpha_1 + A_2^{(2)}\alpha_2 + A_3^{(2)}\alpha_3 + \ldots\ldots + A_s^{(2)}\alpha_s \geqq 0 \\ A_1^{(3)}\alpha_1 + A_2^{(3)}\alpha_2 + A_3^{(3)}\alpha_3 + \ldots\ldots + A_s^{(3)}\alpha_s \geqq 0 \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ A_1^{(r)}\alpha_1 + A_2^{(r)}\alpha_2 + A_3^{(r)}\alpha_3 + \ldots\ldots + A_s^{(r)}\alpha_s \geqq 0 \end{array}\right\}.$$

as the definition of the partitions considered.

If the symbol be $=$ instead of $\geqq$ the solution of the equations falls into the province of linear Diophantine analysis. The problem before us may be regarded as being one of linear partition analysis. There is much in common between the two theories; the problems may be treated by somewhat similar methods, and lead to results of the same general character.

The partition analysis of degree higher than the first, like the Diophantine, is of a more recondite nature, and is left for the present out of consideration.

I treat the partition conditions by the method of generating functions. I seek the summation

$$\Sigma\, X_1^{\alpha_1} X_2^{\alpha_2} X_3^{\alpha_3} \ldots\ldots X_s^{\alpha_s}$$

for every set of values (integers)

$$\alpha_1,\ \alpha_2,\ \alpha_3, \ldots\ldots \alpha_s\quad ,$$

which satisfy the assigned conditions.

It appears that there are, in every case, a finite number of ground or fundamental solutions of the conditions, viz.:—

$$\begin{array}{l} \alpha_1^{(1)},\ \alpha_2^{(1)},\ \alpha_3^{(1)} \ldots\ldots \alpha_s^{(1)} \\ \alpha_1^{(2)},\ \alpha_2^{(2)},\ \alpha_3^{(2)} \ldots\ldots \alpha_s^{(2)} \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ \alpha_1^{(m)},\ \alpha_2^{(m)},\ \alpha_3^{(m)} \ldots\ldots \alpha_s^{(m)} \end{array}$$

such that every solution

$$\alpha_1,\ \alpha_2,\ \alpha_3, \ldots\ldots \alpha_s$$

is such that

$$\begin{aligned}
\alpha_1 &= \lambda_1\alpha_1^{(1)} + \lambda_2\alpha_1^{(2)} \ldots\ldots + \lambda_m\alpha_1^{(m)}\\
\alpha_2 &= \lambda_1\alpha_2^{(1)} + \lambda_2\alpha_2^{(2)} \ldots\ldots + \lambda_m\alpha_2^{(m)}\\
\alpha_3 &= \lambda_1\alpha_3^{(1)} + \lambda_2\alpha_3^{(2)} \ldots\ldots + \lambda_m\alpha_3^{(m)}\\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
\alpha_s &= \lambda_1\alpha_s^{(1)} + \lambda_2\alpha_s^{(2)} \ldots\ldots + \lambda_m\alpha_s^{(m)}
\end{aligned}$$

$\lambda_1, \lambda_2, \ldots\ldots \lambda_m$ being positive integers.

This arises from the fact that every term

$$X_1^{\alpha_1} X_2^{\alpha_2} X_3^{\alpha_3} \ldots\ldots X_s^{\alpha_s}$$

of the summation is found to be expressible as a product

$$\begin{aligned}
&\left\{X_1^{\alpha_1^{(1)}} X_2^{\alpha_2^{(1)}} X_3^{\alpha_3^{(1)}} \ldots\ldots X_s^{\alpha_s^{(1)}}\right\}^{\lambda_1}\\
\times &\left\{X_1^{\alpha_1^{(2)}} X_2^{\alpha_2^{(2)}} X_3^{\alpha_3^{(2)}} \ldots\ldots X_s^{\alpha_s^{(2)}}\right\}^{\lambda_2}\\
\times &\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
\times &\left\{X_1^{\alpha_1^{(m)}} X_2^{\alpha_2^{(m)}} X_3^{\alpha_3^{(m)}} \ldots\ldots X_s^{\alpha_s^{(m)}}\right\}^{\lambda_m}
\end{aligned}$$

Denoting this product by

$$P_1^{\lambda_1} P_2^{\lambda_2} \ldots\ldots P_m^{\lambda_m}$$

the generating function assumes the form

$$\frac{1 - (Q_1^{(1)} + Q_1^{(2)} + Q_1^{(3)} + \ldots) + (Q_2^{(1)} + Q_2^{(2)} + Q_2^{(3)} + \ldots) - (Q_3^{(1)} + \ldots) + \ldots}{(1 - P_1)(1 - P_2) \ldots\ldots (1 - P_m)}$$

wherein the denominator indicates the ground solutions and the numerator the simple and compound syzygies which unite them.

The terms

$$\begin{array}{lll}
Q_1^{(1)},\ Q_1^{(2)},\ Q_1^{(3)} \ldots\ldots & \text{denote first} & \text{syzygies}\\
Q_2^{(1)},\ Q_2^{(2)},\ Q_2^{(3)} \ldots\ldots & \quad ,, \quad \text{second} & ,,\\
Q_3^{(1)},\ Q_3^{(2)},\ Q_3^{(3)} \ldots\ldots & \quad ,, \quad \text{third} & ,,\\
\ldots\ldots\ldots\ldots\ldots\ldots & &
\end{array}$$

The reader will note the striking analogy with the generating functions of the theory of invariants.

Similar results are obtained as solutions of linear Diophantine equations.

The generating functions under view are *real* in the sense of Cayley and Sylvester. Enumerating generating functions of various kinds are obtained by assigning equalities between the suffixed capitals

$$X_1, X_2, \ldots\ldots X_s.$$

Putting, *e.g.*, $X_1 = X_2 = \ldots\ldots = X_s = x$,

we obtain the function which enumerates by the coefficient of x^n, in the ascending expansion, the numbers of solutions for which

$$\alpha_1 + \alpha_2 + \ldots\ldots + \alpha_s = n.$$

It will be gathered that the note of the following investigation is the importation of the idea that the solution of any system of equations of the form

$$A_1\alpha_1 + A_2\alpha_2 + A_3\alpha_3 + \ldots\ldots + A_s\alpha_s \geqq 0$$

(all the quantities involved being integers) is a problem of partition analysis, and that the theory proceeds *pari passu* with that of the linear Diophantine analysis.

"On the Boiling Point of Liquid Hydrogen under Reduced Pressure." By JAMES DEWAR, M.A., LL.D., F.R.S. Received and Read December 15, 1898.

The June number of the 'Proceedings of the Chemical Society' contains a paper by the author on "The Boiling Point and Density of Liquid Hydrogen." A resistance thermometer made of fine platinum wire, called No. 7 Thermometer, was used in the investigation. It had been carefully calibrated, and gave the following resistances at different temperatures:—

Temperature.	Resistance. Ohms.
+99·1° C.	7·337
+75·3	6·859
+51·4	6·388
+25·7	5·857
+0·7	5·338
−78·2	3·687
−182·6	1·398
−193·9	1·136
−214·0	0·690

The zero of the thermometer in platinum degrees was −263·27°. Mr. J. D. Hamilton Dickson, M.A., Fellow of Peterhouse, who contributed a paper to the 'Phil. Mag.' for June, 1898, on "The Reduction

to Normal Air Temperature of the Platinum Thermometers," used in the low temperature researches of Professor Fleming and the author, has been good enough to calculate a special formula for this thermometer No. 7. He finds the formula

$$(R + 43{\cdot}958933)^2 = 2{\cdot}03596488\,(t + 1193{\cdot}1460)$$

expresses the relation between the resistance and temperature in centigrade degrees. This expression gives a probable error of only 0·16° C. over a range of more than 300° C. When this thermometer was placed in boiling hydrogen, the resistance became 0·129 ohm, and remained constant at this value. Calculated into the Dickson formula, this value of the resistance corresponds to a temperature of −238·4° C. If we assume the resistance reduced to zero, then the temperature registered by the thermometer ought to be −244° C. At the boiling point of hydrogen, therefore, if the law correlating resistance and temperature can be pressed to its limits, a lowering of the boiling point of hydrogen by 5° or 6° C. would produce a condition of affairs where the platinum would have no resistance, or become a perfect conductor. Now we have every reason to believe that hydrogen, like other liquids, will boil at a lower temperature the lower the pressure under which it is volatilised. The question arises, how much lowering of temperature can we practically anticipate. For this purpose we have the boiling point and critical data available from which we can calculate an approximate vapour pressure formula, accepting 35° abs. as the boiling point; 52° abs. as the critical temperature, and 19·4 at. as the critical pressure; then as a first approximation

$$\log p = 6{\cdot}8218 - \frac{137{\cdot}9}{T}\ \text{mm.} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1).$$

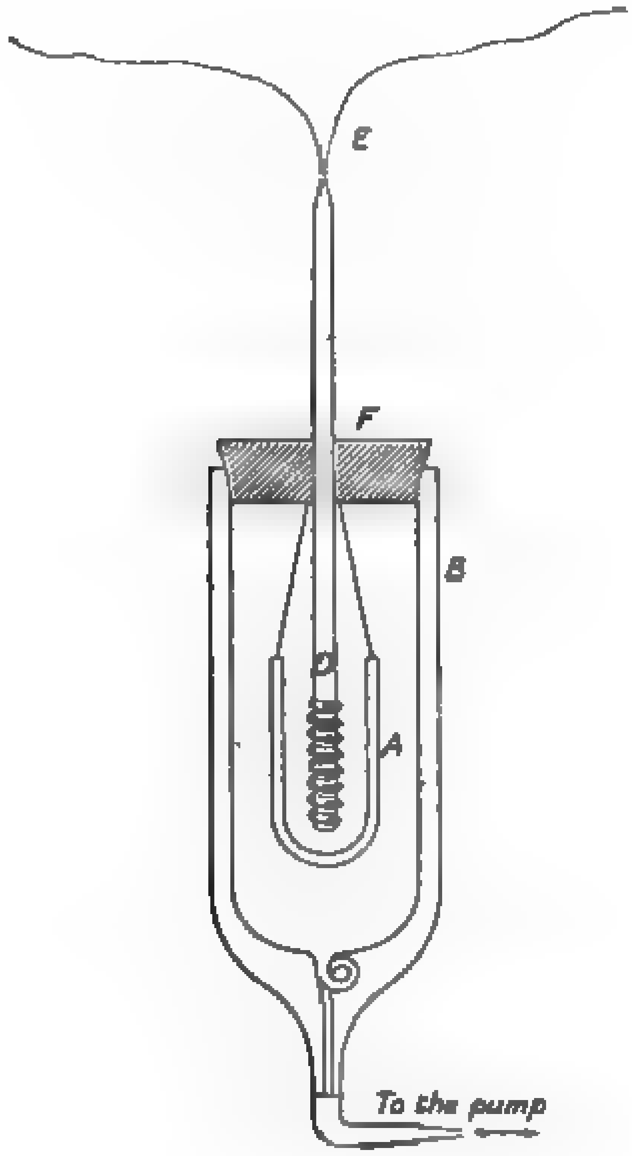
If instead of using the critical pressure in the calculation we assume the molecular latent heat of hydrogen is proportional to the absolute boiling point, then from a comparison with an expression of the same kind, which gives accurate results for oxygen tensions below one atmosphere, we can derive another expression for hydrogen vapour pressures, which ought to be applicable to boiling points under reduced pressure.

The resulting formula is

$$\log p = 7{\cdot}2428 - \frac{152{\cdot}7}{T}\ \text{mm.} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2).$$

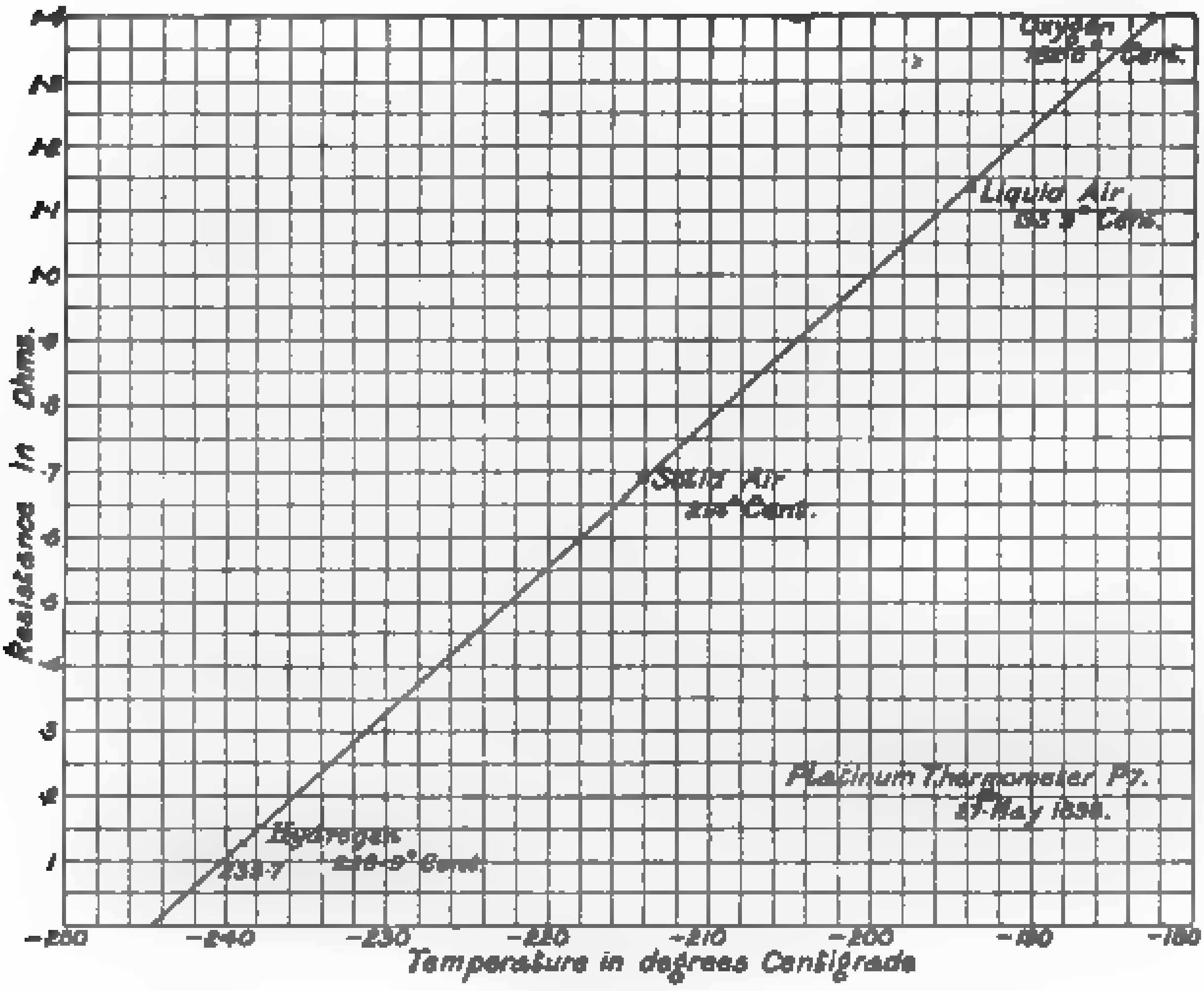
Now formula (1) gives a boiling point of 25·4° abs. under a pressure of 25 mm., whereas the second equation (2) gives for the same pressure 26·1° abs. As the absolute boiling point under atmospheric pressure is 35°, both expressions lead to the conclusion that ebullition

under 25 mm. pressure ought to reduce the boiling point some 10° C. For some time experiments have been in progress with the object of determining the temperature of hydrogen boiling under about 25 mm. pressure, but the difficulties encountered have been so great, and repeated failures so exasperating, that a record of the results so far reached becomes advisable. The troubles arise from the conduction of heat by the leads; the small latent heat of hydrogen volume for volume as compared with liquid air; the inefficiency of heat isolation and the strain on the thermometer brought about by solid air freezing on it and distorting the coil of wire. In many experiments the result has been that all the liquid hydrogen has evaporated before the pressure was reduced to 25 mm., or the thermometer was left imperfectly covered. The apparatus employed will be understood from the figure.



The liquid hydrogen collected in the vacuum vessel A was suspended in a larger vessel of the same kind B, which is so constructed that a spiral tube joins the inner and outer test-tubes of which B is made, thereby making an opening into the interior at C. The resistance thermometer D and leads E pass through a rubber cork F, and the

exhaustion takes place through C. In this way the cold vapours are drawn over the outside of the hydrogen vacuum vessel, and this helps to isolate the liquid from the connective currents of gas. To effect proper isolation the whole apparatus ought to have been immersed in liquid air under exhaustion. Arrangements of this kind add to the complication, so in the first instance the liquid was used as described. The liquid hydrogen evaporated quietly and steadily under a pressure of about 25 mm. of mercury, without the least appearance of solidification or loss of mobility; still remaining clear and colourless to the eye. Naturally the liquid does not last long, so the resistance has to be taken quickly. Just before the reduction of pressure began, the resistance of the thermometer was 0·131 ohm. This result compares favourably with the former observation on the boiling point, which gave a resistance of 0·129 ohm. On reducing the pressure, the resistance diminished to 0·114 ohm, and kept steady for some time. The lowest reading of resistance was 0·112 ohm. This value corresponds to $-239·1°$ C., or only one degree lower than the boiling point at atmospheric pressure, whereas the temperature ought to have been reduced some 10° C. or in any case 5° under the assumed exhaustion. The position of the observation on the curve of the relation of temperature and resistance for No. 7 thermometer is shown on the accompanying diagram. The question arises then as to what is the explanation of



this result? Has the platinum resistance thermometer arrived at a limiting resistance about 35° abs., so that at a lower temperature it refuses to change in resistance, the curve having become practically asymptotic to the axis of temperature? On the other hand, has the influx of heat by the leads, and the correction on account of this change of resistance, become so great as to vitiate the results at these excessively low temperatures? Again, it may be suggested that the thermometer was not properly cooled, or that the liquid hydrogen does not lower in temperature to any marked extent under exhaustion like other liquids. All these conjectures can only be set at rest by a repetition of the experiments with a new thermometer of much higher initial resistance, and under conditions of better heat isolation. No blunder having been detected in the observations, for the present we must assume that the platinum resistance thermometer No. 7 acts in the manner described. It would be premature to discuss the inferences to be drawn from these results until they are confirmed on another variety of platinum wire made into a resistance thermometer. But as this will involve the use of considerable quantities of liquid hydrogen, it will take some time to complete the investigation.

The same kind of anomaly appears in the case of the use of a thermo-junction at these low temperatures, but this is a separate matter, and must be dealt with in a further communication.

I am indebted to Mr. J. E. Petavel for assistance in the electrical measurements, and also to Mr. Robert Lennox and Mr. Heath for their general help in the conduct of the experiments.

"Application of Liquid Hydrogen to the Production of High Vacua, together with their Spectroscopic Examination." By JAMES DEWAR, M.A., LL.D., F.R.S. Received and Read December 15, 1898.

As an illustration of the extraordinary power of the new cooling agent—liquid hydrogen, the extreme rapidity with which high vacua can be produced by its use is, perhaps, one of the most striking. The absolute boiling points of hydrogen, oxygen, and chlorine are respectively 35°, 90° and 240°, in other words oxygen boils at a temperature two and a half times higher than liquid hydrogen, and liquid chlorine similarly at two and a half times that of liquid oxygen. From this we infer that liquid hydrogen as a cooling agent ought to be relative to liquid air as effective as the latter is compared to that of liquid chlorine. Now chlorine at the temperature of boiling oxygen is a hard solid, some 80° below its melting point, and in this condition has an excessively feeble vapour pressure. When liquid hydrogen freezes air out of a sealed tube by immersing the end in the liquid, it is to be

inferred that no measurable pressure of air ought to be left in the vessel. If we apply Van der Waal's law of corresponding temperatures to the case of hydrogen, the above inference is made unimpeachable. An approach to some knowledge of what the tension of air must be about the boiling point of hydrogen can be attained by extrapolating the vapour pressure curves of oxygen and nitrogen. Taking the following range of boiling point temperatures for nitrogen and oxygen, viz., from the critical point to the boiling point under diminished pressure, two Willard Gibbs formulæ were calculated, with the following results :—

Nitrogen	Temp. abs.	127°	78·6°	59°
	Pressure in mm.	25,900	740	26

$$\text{Nitrogen.} \quad \log_{10}p = 11{\cdot}5561 - \frac{400{\cdot}02}{T} - 1{\cdot}8980 \log_{10}T \ldots\ldots \quad (1).$$

Oxygen	Temp. abs.	154°	90·3°	61·3°
	Pressure in mm.	37,592	740	7·5

$$\text{Oxygen.} \quad \log_{10}p = 9{\cdot}4699 - \frac{422{\cdot}22}{T} - 0{\cdot}9843 \log_{10}T \ldots \quad (2).$$

Another Gibbs formula was calculated, taking Estreicher's values for the vapour pressure of liquid oxygen below its boiling point, viz. :—

Temp. abs.	91·44°	78·1°	62·8°
Pressure in mm.	743·8	141·8	7·5

$$\text{Oxygen.} \quad \log_{10}p = 16{\cdot}0670 - \frac{524{\cdot}72}{T} - 3{\cdot}8024 \log_{10}T \ \ldots \quad (3).$$

We deduce from these formulæ the following vapour pressures at the temperature of boiling hydrogen :—

(1) Nitrogen	0·0015	Pressure in mm., 35° abs.
(2) Oxygen	0·000076	do.
(3) ,,	0·000016	do.

The results of calculation, taking the formulæ for the widest range of pressures, viz., (1) and (2), may probably be the surest, but in any case those values must be taken as a *maximum*, seeing they refer to the liquid state, while both oxygen and nitrogen, at the temperature of 35° absolute, are hard solids, and must therefore have dropped to lower tensions than that of the extrapolated liquid vapour pressure curves. It is curious to note that at this low temperature the *theoretical* ratio of the tensions of nitrogen and oxygen is as 20 to 1. *Direct measurements* of the vapour pressure of nitrogen at the melting

point, or 60° absolute, gave the value of 26 mm., and a ratio of the tensions of nitrogen to oxygen of 6 to 1, whereas from the curves the value ought to be 6·7 to 1. Olszewski gives the tension of nitrogen at $-214°$ as 60 mm., and as at this temperature the oxygen tension is 3·8 mm., the ratio of the saturated pressures of the two gases at the melting point of nitrogen would be as 16 to 1, which is far too high. Probably the oxygen value will be nearest the truth, seeing it has the lowest melting point. The tension is about a ten millionth of an atmosphere. In the case of nitrogen, the maximum theoretical pressure would be one five-hundred-thousandth of an atmosphere. It is safe to infer that the vacuum left after liquefying the air out of a vessel by means of liquid hydrogen cannot exceed the millionth part of the atmospheric pressure, exclusive of the pressure resulting from any incondensable material other than nitrogen and oxygen. This is just about the pressure of the vapour of mercury at the ordinary temperature in the Torricellian vacuum, so that as good an exhaustion ought to result as can be got by boiling out a space with mercury. There is another way in which the question may be put. Assuming the molecular latent heats are approximately proportioned to the absolute boiling points, then we can, from a comparison with the oxygen value, deduce that of hydrogen, and thereby get the constants in a two-term formula for the vapour pressures. For pressures below an atmosphere, the following approximate formulæ were deduced :—

$$\text{Oxygen} \ldots\ldots \quad \log p = 7{\cdot}2058 - \frac{392{\cdot}6}{T} \text{ mm.} \quad \ldots\ldots \quad (4).$$

$$\text{Hydrogen} \ldots \quad \log p = 7{\cdot}2428 - \frac{152{\cdot}7}{T} \text{ mm.} \quad \ldots\ldots\ldots \quad (5).$$

From these expressions it follows that at its boiling point, or 35° absolute, hydrogen has 7/852000 times the pressure of oxygen, or the latter pressure is about the eight millionth of an atmosphere. A similar formula, calculated from the critical and boiling point data, gives substantially the same order of quantities. Formula (4) for oxygen tensions must be fairly accurate, seeing it gives a theoretical latent heat of about 56 units per gram of liquid evaporating at the boiling point, whereas direct determinations result in 55 units. To test this inference, the following plan of experimenting was adopted :— Ordinary shaped vacuum tubes, like A, B, used for the spectroscopic examination of gases, with and without electrodes (figs. 1 and 2), having a capacity ranging from 15 to 25 c.c., had pieces of quill tubing about a foot long sealed on. The tubes were contracted at D to about 1 mm., so that they could be sealed off with rapidity. The end C sometimes terminated in a small bulb (fig. 3), in order to give increased cooling surface, and, when necessary, to allow many times the volume

Fig. 1.

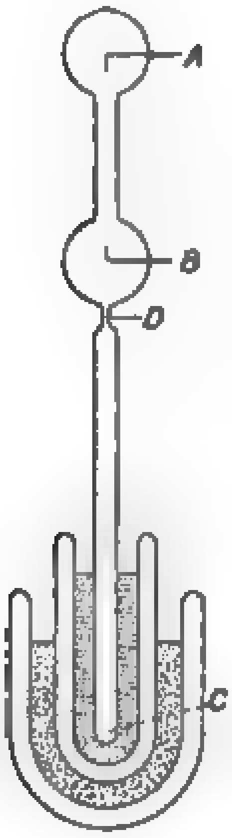


Fig. 2.

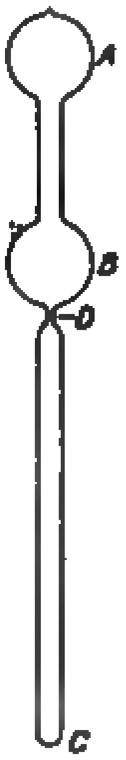
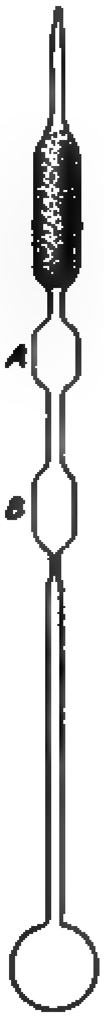


Fig. 3.



of air in A, B, to enter and be condensed with the object of accumulating any incondensable residuum.

The tubes were filled with air, oxygen, and nitrogen at the atmospheric pressure. The liquid hydrogen collected in the vacuum vessel, immersed in another similar vessel full of liquid air, being ready, the end C was dipped in the liquid for a little over a minute, and the tube AB sealed off at D, so that on removal from the hydrogen bath the solid air might melt and distil off without generating any pressure. On attempting to pass the spark through vacuum tubes prepared in this manner, their excellent exhaustion was revealed by great resistance to the passage of the discharge, and the high phosphorescence of the glass. Two tubes, kindly prepared by Sir William Crookes with platinum electrodes that he had previously sparked to remove gases and impurities on the glass before filling with dry air, gave, when treated in the manner described, such high vacua that the tubes had to be heated in order to get any spark to pass. Thus it is proved that the tension of solid nitrogen and oxygen at the temperature of boiling hydrogen is below the millionth of an atmosphere, seeing there is less difficulty in getting a discharge to pass in tubes exhausted to this extent. In order to get some definite idea of the limit of the exhaustion produced, two tubes, such as have been described as suitable for

the liquid hydrogen experiments, might be joined together and filled with oxygen or nitrogen at atmospheric pressure, and simultaneously exhausted with the mercurial pump to a small fraction of an atmosphere, and then sealed off from the pump and each other. One of these two identical tubes could then be subjected to the hydrogen cooling, following the directions already given, and the two vacuum tubes now compared. If there was a marked difference in resistance to the passage of the discharge in the frozen tube, then something must have condensed, and by a few tentative trials a limit might be reached when the initial exhaustion was unaffected by the hydrogen cooling. Such experiments have not yet been made. The presence of any vapour of mercury would require to be carefully eliminated, otherwise the method would not be satisfactory. Tubes that are prepared without taking special precautions to exclude organic matter and water from the glass, deteriorate, especially with electrodeless tubes after the discharge has taken place for some time.

The rapidity with which the vacua are attained is such as theory would suggest; assuming a hole of a square millimetre in section through which the air rushes into the condenser, and that a velocity of current between 600 and 700 feet a second is attained, then a vessel of 20 c.c. capacity could be reduced in pressure in 1 second to 1/10 of the initial pressure, and if the same rate is continued at the end of 60 seconds to $(\frac{1}{10})^{60}$. Sir George Stokes has been good enough to consider the problem and writes as follows :—

"Let V be the volume of the vessel, A the area of an aperture by which the air is conceived as rushing out with the velocity v, ρ the density of the air in the vessel at the time t, D the initial density, that is, the atmospheric density.

"Then, according to our hypothesis, $Av.dt$ is the volume of air, and, therefore, $Av\rho.dt$, the mass of air, which rushes out in the time dt. But this equals the loss of mass of air in the vessel during the time dt, and, therefore,

$$Av\rho\,.\,dt = -Vd\rho,$$

a differential equation of which the integral is

$$\rho = De^{-Avt/V}.$$

"Suppose now V to be 20 c.c., or 20,000 c.mm., A to be the area of a circle of 1 or 2 mm. diameter, say 2 sq. mm., v to be 333 m., or 333,000 mm., t (in seconds) to be 60; then

$$\log_e \frac{D}{\rho} = \frac{2 \times 333{,}000 \times 60}{20{,}000} = 1998,$$

$$\frac{D}{\rho} = 5254 \times 10^{434}.$$

"This would give a density of almost inconceivable smallness. Doubtless the supposition made above as to the rate of discharge is very wide of the mark, being much too great. If the velocity of rushing is about half the velocity of sound, the ratio of densities would become 72×10^{217}. If so it is satisfactory to find that the mathematical following out of the hypothesis leads to a density of the residual air in the vessel which is enormously below what suffices to account for the observed result." A practical mode of rapidly attaining a high vacuum in any vessel is to displace the air with carbonic or sulphurous acid, either at the atmospheric or under diminished pressure, and then to freeze out the remaining gas by the use of liquid air, just as in the experiments with liquid hydrogen.

The first vacuum tube was an electrodeless one, the air had not been dried, nor the glass specially cleaned. On spectroscopic examination it showed hydrogen lines bright along with the second or compound line spectrum of the same gas, and a series of bright bands defined on the less refrangible side, diffuse on the more refrangible, which occur in the yellow, green, blue, and indigo. These bands were found to be identical with the carbonic oxide spectrum. With a Leyden jar in the secondary circuit the line spectrum of hydrogen disappeared, leaving the second spectrum fainter; but the carbonic oxide bands remained bright, and there was no appearance of the hydrocarbon spectrum. The second tube had aluminium electrodes, and, like the last, had no special treatment in filling in the air. This tube showed also the line spectrum and the second spectrum of hydrogen; the latter being bright along with the carbonic oxide spectrum; but on sparking the latter disappeared. No appearance of the hydrocarbon spectrum could be detected, but there was a suspicion of bands in the indigo like the negative pole spectrum of nitrogen. The addition of a Leyden jar brought out nothing new, only intensifying the line spectrum of hydrogen, while leaving the second spectrum bright. In neither of the above tubes could any lines of nitrogen or oxygen be recognised. The third tube was filled with air drawn over cotton wool, red-hot copper oxide, and phosphoric pentoxide, no rubber joints being employed. The spectrum showed the carbonic oxide bands and the hydrogen line spectrum as before. Only the second hydrogen spectrum was feeble. There was a yellow line W.L. 5849, identical with one occurring in the natural gas from the King's Well at Bath. In a paper on "The Liquefaction of Air and the Detection of Impurities,"* the separation of helium from this gas is described by liquefaction and fractionation, and it was observed that during the sparking the helium lines were well marked along with "*others, the origin of which must be settled later.*" It was further observed, "*With a modified form of apparatus it will be possible to collect any residuary gas from the use not of*

* 'Chem Soc. Proc.,' November, 1897.

3 cubic feet of air or Bath gas, but from hundreds of cubic feet of such products." The helium and other associated material was shown to be more volatile than nitrogen. Pursuing this course of investigation in the summer of this year, the volatile portion of air was examined, when the presence of material giving the same lines as Bath helium was recognised. While this investigation was in progress, Professor Ramsay and Dr. Travers observed the same spectrum in the more volatile portion of argon which they have associated with a new element called neon. The use of liquid hydrogen, as described, proves that the most characteristic line of neon in the yellow, about W.L. 5849, can be detected in 25 c.c. of ordinary air, and the presence of helium in the atmosphere is confirmed.*

A fourth tube, filled like the preceding one, had a phosphoric pentoxide tube left on. This showed again the carbonic oxide bands, but no hydrogen lines could be detected; while the oxide of copper ought to have removed all free hydrogen and transformed all the organic matter into carbonic acid and water. Yet it appears that the spectrum of the carbon compounds is difficult to remove from electrodeless tubes, probably owing to carbonic acid coming from the glass. There were some broad diffuse bands that may arise from the drying agent. The absence of hydrogen in this tube suggests that its presence in the third tube was due to vapour of water coming slowly from the glass. I am greatly indebted to Professor Liveing for making a careful examination of the spectra of these tubes.

Sir William Crookes was good enough to prepare two tubes with platinum electrodes, which he sparked in vacua till all hydrogen disappeared, and then filled with dry air, but without the use of red-hot copper oxide or any agent for the absorption of carbonic acid or the destruction of organic matter. After the cooling with liquid hydrogen, he found on spectroscopic examination, in one no hydrogen, but two faint lines, one about 5852 W.L. and the other 5676 W.L. The second tube showed the same yellow about 5852, the helium line along with 5939 and 6145, the hydrogen lines C and F, and some red lines. The observations of Crookes confirm the presence of neon, helium, and hydrogen. The absence in his tubes of the carbonic oxide spectrum is important, seeing all the electrodeless tubes gave this spectrum. In these tubes the vacuum was very high, and it was difficult to observe the gaseous spectrum. Still, the fact of finding hydro-

* In a paper along with Professor Liveing, "On the Spectra of the Electric Discharge in Liquid Oxygen, Air, and Nitrogen," 'Phil. Mag.,' 1894, we noted that during the distillation and concentration *in vacuo* of liquid oxygen and air under diminished pressure that two bright lines appeared in the spectrum at wave-lengths 557 and 555, and that one of these lines was very near the position of the auroral line. These lines are now attributed by the same chemists to a new element, crypton.

gen in one and not in the other, leaves the presence of free hydrogen in the atmosphere as a question for further inquiry. The tube that did not contain hydrogen was heated very hot in order to get a discharge, and then the spectrum showed some bands like the negative glow of nitrogen. Occasionally, a jar discharge was got to pass, and when this took place the nitrogen lines could be seen. An electrodeless tube filled carefully with oxygen made from fused chlorate of potash, which was contained in an extension of the vacuum tube gave nothing but the carbonic oxide bands. In future experiments it will be easy to concentrate all the most volatile material in air or other gases, and thereby to make a more thorough examination of the spectrum. In the meantime my object is to show one of the scientific uses of liquid hydrogen.

I have to thank Mr. Robert Lennox for efficient aid in the conduct of the difficult experiments. Mr. Heath has also helped in the work.

January 19, 1899.

The LORD LISTER, F.R.C.S., D.C.L., in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Observations upon the Normal and Pathological Histology and Bacteriology of the Oyster." By Professor W. A. HERDMAN, F.R.S., and Professor R. BOYCE.

II. "On the Formation of Multiple Images in the Normal Eye." By SHELFORD BIDWELL, F.R.S.

III. "On the Vibrations in the Field round a Theoretical Hertzian Oscillator." By Professor KARL PEARSON, F.R.S., and Miss ALICE LEE.

IV. "On the Refractive Indices and Densities of Normal and Semi-normal Aqueous Solutions of Hydrogen Chloride and the Chlorides of the Alkalis." By Sir JOHN CONROY, Bart., F.R.S.

"Observations upon the Normal and Pathological Histology and Bacteriology of the Oyster." By W. A. HERDMAN, D.Sc., F.R.S., Professor of Zoology in University College, Liverpool, and RUBERT BOYCE, M.B., Professor of Pathology in University College, Liverpool. Received December 24, 1898,—Read January 19, 1899.

(Abstract.)

This research was commenced three years ago, and has been carried on intermittently in the intervals of other work.

Preliminary reports on some of our results have been laid before the British Association at the Ipswich, Liverpool, Toronto, and Bristol meetings, and a short paper on one section of the subject was communicated to the Royal Society and printed in the 'Proceedings' last year. In the present paper we give a full account, with illustrations, of the detailed evidence upon which our various conclusions are based. The following is a brief statement of the more important results given in the paper :—

1. Although our primary object was to study the oyster under unhealthy conditions, in order to elucidate its supposed connection with infective disease, we found it necessary to study in minute detail the histology of certain parts of the body, especially the gills and mantle lobes, the alimentary canal and liver. We give figures and descriptions of these structures in both normal and abnormal conditions.

2. We have also worked out the distribution and probable function of a minute muscle, which we believe to be the modified representative of the protractor pedis muscle of some other molluscs.

3. A diseased condition we found in certain American oysters very soon brought us into contact with the vexed question of the "greening" of oysters, and one of the first results we arrived at was that there are *several distinct kinds* of greenness in oysters. Some of them, such as the green Marennes oysters, and those of some rivers on the Essex coast, are healthy; while others, such as some Falmouth oysters, containing copper, and some American oysters re-bedded on our coast, and which have the pale-green "leucocytosis" described in our former paper to the Royal Society, are not in a healthy state.

4. Some forms of greenness (*e.g.*, the leucocytosis) are certainly associated with the presence of a greatly increased amount of copper in the oyster, while other forms of greenness (*e.g.*, that of the Marennes oysters) have no connection with copper, but depend upon the presence of a special pigment, "marennin."

We are able, in the main, to support Ray Lankester in his observa-

tions on Marennes oysters; but we regard the wandering amœboid granular cells on the surface of the gills as leucocytes which have escaped from the blood spaces, and have probably assumed a phagocytic function.

5. We see no reason to think that any iron which may be associated with the marennin in the gills, &c., is taken in through the surface epithelium of the gill and palps, but regard it, like the rest of the iron in the body, as a product of ordinary digestion and absorption in the alimentary canal and liver.

6. We do not find that there is any excessive amount of iron in the green Marennes oyster compared with the colourless oyster, nor do the green parts (gills, palp, &c.) of the Marennes oyster contain either absolutely or relatively to the colourless parts (mantle, &c.) more iron than colourless oysters. We therefore conclude that there is no connection between the green colour of the "Huîtres de Marennes" and the iron they may contain.

7. On the other hand, we do find by quantitative analysis that there is more copper in the green American oyster than in the colourless one; and more proportionately in the greener parts than in those that are less green. We therefore conclude that their green colour is due to copper. We also find a greater quantity of iron in those green American oysters than in the colourless; but this excess is, proportionately, considerably less than that of the copper.

8. In the Falmouth oysters, containing an excessive amount of copper, we find that much of the copper is certainly mechanically attached to the surface of the body, and is in a form insoluble in water, probably as a basic carbonate. In addition to this, however, the Falmouth oyster may contain a much larger amount of copper in its tissues than does the normal colourless oyster. In these Falmouth oysters the cause of the green colour may be the same as in the green American oyster.

9. By treating sections of diseased American oysters under the microscope with potassium ferrocyanide and various other reagents, we find that the copper reactions correspond in distribution with the green coloration; and we find, moreover, from these micro-chemical observations that the copper is situated in the blood-cells or leucocytes, which are greatly increased in number. This condition may be described as a green leucocytosis, in which copper in notable amount is stored up in the leucocytes.

10. We find that an aqueous solution of pure hæmatoxylin is an extremely delicate test for copper, just as Macallum found it to be for iron.

11. Experiments in feeding oysters with weak solutions of various copper and iron salts gave no definite results, certainly no clear evidence of any absorption of the metals accompanied by "greening."

12. Although we did not find the *Bacillus typhosus* in any oysters obtained from the sea or from the markets, yet in our experimental oysters inoculated with typhoid we were able to recover the organism from the body of the oyster up to the tenth day. We show that the typhoid bacillus does not increase in the body or in the tissues of the oyster, and our figures indicate that the bacilli perish in the intestine.

13. Our experiments showed that sea-water was inimical to the growth of the typhoid bacilli. Although their presence was demonstrated in one case on the twenty-first day after addition to the water, still there appeared to be no initial or subsequent multiplication of the bacilli.

14. In our experiments in washing infected oysters in a stream of clean sea-water the results were definite and uniform; there was a great diminution or total disappearance of the typhoid bacilli in from one to seven days.

15. The colon group of bacilli is frequently found in shell-fish as sold in towns, and especially in the oyster; but we have no evidence that it occurs in mollusca living in pure sea water. The natural inference that the presence of the colon bacillus invariably indicates sewage contamination must, however, not be considered established without further investigation.

16. The colon group may be separated into two divisions: (1) those giving the typical reactions of the colon bacillus, and (2) those giving corresponding negative reactions, and so approaching the typhoid type; but in no case was an organism giving all the reactions of the *B. typhosus* isolated. It ought to be remembered, however, that our samples of oysters, although of various kinds and from different sources, were in no case, so far as we are aware, derived from a bed known to be contaminated or suspected of typhoid.

17. We have shown also the frequent occurrence, in various shell-fish from the shops, of anaërobic spore-bearing bacilli giving the characteristics of the *B. enteritidis sporogenes* recently described by Klein.

18. As the result of our work, we make certain recommendations as to the sanitary regulation and registration of the oyster beds, and as to quarantine for oysters imported from abroad.

"On the Formation of Multiple Images in the Normal Eye." By SHELFORD BIDWELL, M.A., LL.B., F.R.S. Received December 8, 1898—Read January 19, 1899.

[PLATE 5.]

It is well known that a small bright object for which the eye is not accommodated often presents a multiform appearance, the number of *separate images* perceived varying in different cases from about six to

fifteen. In Helmholtz's 'Physiological Optics,' drawings are given illustrative of the phenomena exhibited by a luminous point when the conjugate focus is situated a little in front of or a little behind the retina. A narrow luminous line such as that formed when a spectroscope slit is held before a flame or other bright background may become similarly multiplied. These and other allied phenomena are believed to arise from the suture-like radial lines, six or more in number, which occur upon the two surfaces of the crystalline lens.

It is also known that as the result of disease or malformation of the eye, the patient may habitually see several images of single objects. But in the course of a careful search among physical and physiological publications, in which Dr. Dawson Turner, of Edinburgh, has most kindly assisted me, I have been unable to find any reference to certain curious phenomena of vision which attracted my notice in the year 1897, and which form the subject of the present communication. It appears that under suitable conditions a normal healthy eye can see hundreds of independent images of a single point, an effect which probably results from the cellular structure of the lenses and the membranes associated with them.

In the earlier observations, the object consisted of a small bright disc. Several different discs were employed, their diameters ranging from 0·5 mm. to 8 mm., and other details were also varied, but (when the circumstances were suitably modified) the results were in all cases of the same nature. It will be convenient to describe the procedure actually carried out in a particular experiment; but, except as furnishing a rough guide for the repetition of the experiment, no special importance must be attached to the distances mentioned; they vary greatly for different individuals, and from time to time even for the same eye.

The condenser of a lantern was covered with two sheets of glass, the one ground and the other deep red. In front of these was placed a brass plate, in the middle of which was drilled a hole $\frac{1}{12}$ inch (2 mm.) in diameter. Inside the lantern was an incandescent electric lamp of 25-candle power. The observer, standing with his left eye at a distance of about 2 feet from the hole in the plate, first covered the hole with a concave lens of 11 inches (28 cm.) focal length, held in his hand, and then slowly moved the lens towards his eye. When the lens was some four or five inches away from the hole, the outline of the little bright disc began to appear multiple; there seemed, in fact, to be a number of little discs almost, but not quite exactly, superposed. As the lens approached the eye, the images became gradually more and more widely dispersed, and when the eye was reached, they had become completely separated. There now appeared to be seven bright discs—a central one surrounded by six others, their arrangement being fairly symmetrical; these were backed by an irregular luminous haze

of nearly circular outline. If the light was made stronger, each of the circumferential discs acquired a pointed tail, directed radially outwards, and the whole appeared like a six-rayed star. So far there is little or nothing new in the observation.

The observer then gradually moved backwards, still holding the lens at his eye; the outer discs at once began to elongate radially, and each soon became resolved into two or more, the approximate symmetry of the figure being still retained. When the distance from the eye to the hole was 3 feet, the number of images that could be counted was about twenty, and the appearance presented was happily likened, by an expert person who confirmed my observations, to that of a large unripe blackberry. If an orange-yellow glass were substituted for the dark red one, the stronger illumination again gave rise to the development of tails, and the blackberry became transformed into a beautiful flower. At 4 feet distance the images had increased to about forty, which was nearly the greatest number that could be counted with any degree of certainty. But, while becoming much less easily distinguishable, they still obviously continued to multiply. At 25 feet there was seen a mottled luminous patch streaked with a few bright lines, evidently corresponding with the sutures of the crystalline lens. These bright lines were found to consist of overlapping images of the round hole, and traces of many similar images could be detected in different parts of the mottled patch.

The above described effects can be observed equally well and with but little modification when the lens employed is convex instead of concave; indeed, any one who is skilled in the mangement of his eyes may dispense with the lens altogether.

I have tried to describe the phenomena as seen with my left eye. With the right eye they are of the same general character, but differ in details; in particular, the separate images first seen are less symmetrically arranged, and their number appears to be eight instead of seven. The observations in question would be found difficult or impossible by a novice in optical experiment, partly on account of his inability to keep his eye in a definite state of accommodation, but chiefly perhaps because he would not recognise what he saw.

I thought that the observations might be rendered easier if the source of light had a more distinctive and conspicuous form than that of a simple circle. Experiments were, therefore, made with a semicircular hole, and this was in some respects an improvement; but far better results were afterwards obtained by using as a source of light the horseshoe-shaped filament of an electric lamp, screened by a coloured glass. When such a lamp was looked at through a lens, concave or convex, of about 6 inches focal length, from a distance of a few feet, the roughly oval patch of luminosity formed upon the retina appeared to be made up of a crowd of separate images of the filament, some

being brighter than others, as represented in fig. 1. The number was apparently few when the observer was near the lamp, and greatly increased as he retired from it, or moved the lens further from his eye.

It occurred to me that the analysis of the luminous field would be facilitated if the attention could be confined to a small portion of it. With this object in view I interposed an adjustable slit taken from a spectroscope, between the eye and the lens, and adjusted its width by trial. When the round hole in the brass plate was viewed through this arrangement it appeared like a string of bright beads, arranged not in a perfectly straight line but somewhat sinuously. A slight movement of the slit in a direction perpendicular to its length produced a curious wavelike motion of the beads.

By sufficiently increasing the distance between the source of light and the eye, perhaps as many as twenty-four or twenty-five bright spots might be made to appear in the row, but they could not be counted with certainty. At a greater distance, or with a lens of shorter focus, the spots became indistinct and blurred.

The appearance presented by the filament of the electric lamp, when seen through the slit, made $\frac{1}{80}$ inch (0·3mm.) wide, and a convex lens of 5 inches (12·7 cm.) focus is very well imitated in figs. 2, 3, and 4, which show the effect when the slit is in horizontal, vertical, and intermediate positions. The imitation was produced by photographing the lamp by means of a lens covered with two layers of gauze, the one containing seventy-five meshes to the linear inch, the other fifty; a slit $\frac{1}{25}$ inch (1 mm.) in width was placed before the lens.

Another attempt was made to count the number of images in a row. The whole of the filament was screened from view, except a very short portion of one limb, which was viewed from a distance of about 8 feet (2·5 m.) through the spectroscope slit, and a convex lens of 5 inches (12·7 cm.) focus. A sheet of coloured glass was interposed as before, and care was taken to hold the slit in such a position that the length of the row was a maximum. According to the estimates of five different observers, the number of images ranged from twenty to thirty. One excellent observer counted them several times, his greatest total being twenty-seven, and his smallest twenty-three. Exact enumeration is perhaps impossible, for though at the first glance one receives the impression that the number of images is quite definite, and probably about twenty-five, closer examination shows that it is often very difficult to localise the line of demarcation between successive images.

The number of images in a row varies with the dilatation of the pupil. If a lighted candle be held near the eye with which the observation is not being made, the pupil of the observing eye contracts sympathetically, and two or three images disappear from each end of the row.

FIG. 1.

FIG. 2.

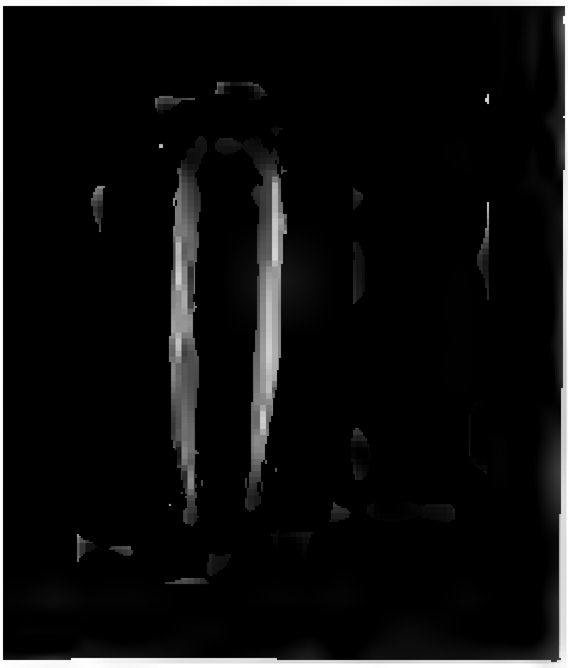


FIG. 3.

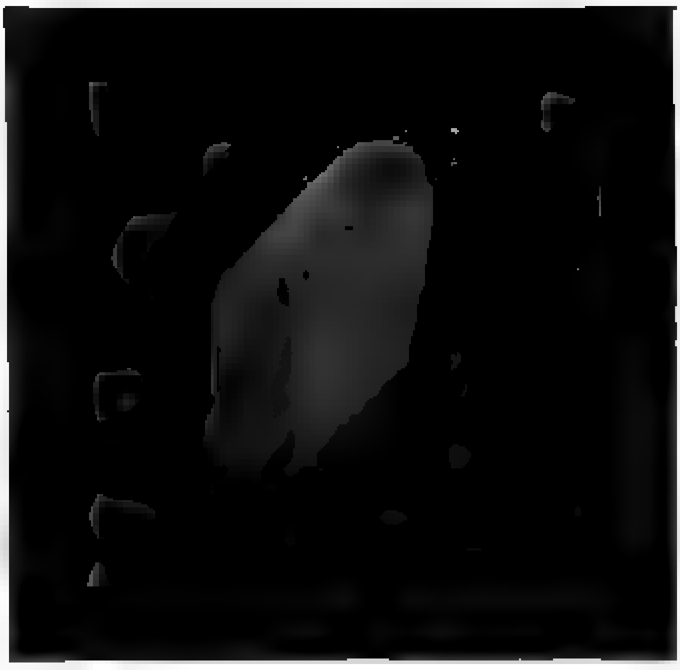


FIG. 4.

If the distance between the eye and the incandescent filament is made much more than 8 feet, or if a lens of shorter focus is employed, the multiple images become blurred and indistinct; at a distance of 20 feet with a lens of 3 inches (7·6 cm.) focal length, the separate images appeared to have coalesced, but the band of light was crossed by a very large number of hazy dark lines at right angles to its length and at fairly equal distances apart.

Thinking that the original images had been resolved into still simpler elements, I endeavoured to ascertain how many elements were developed from each image. Fixing my attention upon a conspicuous image near the end of the row, I moved a convex lens slowly forwards in front of the slit, and carefully watched the changes which occurred. It was found very difficult to follow them satisfactorily, but the conclusion arrived at was that the space corresponding to a single image was ultimately crossed by from fifteen to twenty dark lines; hence, assuming twenty-five images, the total number of elements would be four or five hundred.

Taking the diameter of the pupil when feebly illuminated to be $\frac{1}{8}$ inch, these latter observations seem to indicate some fairly regular anatomical structure in or near the crystalline lens, and composed of cells measuring about $\frac{1}{2500}$ inch (0·01 mm.) in length or breadth. It has been suggested to me that the cause may be found in the endothelium on the anterior surface of the lens, the cells of which are polyhedral and flattened, and about 0·02 mm. in diameter. Their dimensions appear to be too large, but perhaps the agreement is as close as could be expected.

I do not know of any structure sufficiently coarse-grained to account for the images of which twenty-five or thereabouts occur in a row. The mesh of a network which would explain these should be about $\frac{1}{125}$ inch (0·2 mm.) in length, and nothing of the kind is, I believe, to be found in the eye. Probably, however, the effect is a composite, or rather a differential one, like that of the two pieces of gauze used in photographing the lamp. If light passed through two or more superposed nets having fine meshes, dark bands would generally be produced, which would take the form of a network of a coarser mesh than those of the nets themselves—possibly much coarser, as would be the case if the two nets were nearly alike in structure.

The seven or eight images referred to in the description of the first observation are, as before mentioned, undoubtedly due to the sections or sutures of the lens.

"On the Vibrations in the Field round a Theoretical Hertzian Oscillator." By KARL PEARSON, F.R.S., and ALICE LEE, B.A., B.Sc., University College, London. Received January 2,—Read January 19, 1899.

(Abstract.)

(1) The object of this paper is to investigate the types of wave motion in the neighbourhood of a *theoretical* Hertzian oscillator. By a theoretical Hertzian oscillator the writers understand a Maxwellian "double point" of initial maximum moment $\pm El$. But as the actual oscillator has been shewn by Bjerknes and others to give a damped wave train, we take the maximum moment to run down with the time, and to oscillate between the limits $\pm Ele^{-p_1t}$. This gives a wave train corresponding to that observed by Bjerknes and represented at a given distance by

$$Ce^{-p_1t}\sin(p_2t+\gamma).$$

The investigation for a "double point" with a *steady* wave train was originally made by Hertz himself, and has found its way into most of the current text-books of electro-magnetism. The theory thus given, is insufficient for two reasons, both of which were recognised by Hertz himself, namely, because (i) the actual oscillator has sensible extension, and (ii) the wave train it gives forth is not steady.

The present paper only attempts to remove the latter objection to Hertz's original theory; like that theory it becomes less accurate as we approach nearer to an actual oscillator. Still the range within which the damping produces a very sensible divergence from Hertz's theory, seems sufficiently large to allow of experiment being made at a considerable distance from the oscillator; certainly the chief divergences between the present and Hertz's original theory actually fall in the portion of the field, wherein his chief interference experiments were made. Besides therefore the difficulties arising from the phenomena of "multiple resonance," it seems necessary to measure the influence of damping in modifying the mathematical results for a steady wave train, which results are what Hertz made use of in interpreting his interference experiments. The four sources of divergence between theory and experiment in Hertz's case, *i.e.*:

(i) the damping of the wave train,
(ii) the size of the oscillator,
(iii) multiple resonance,
(iv) defect of electro-magnetic theory,

may one or all be effective, but the object of the present paper is *confined entirely to* a theoretical investigation of the first.

(2) After the writers had investigated the general theory of a double point with damped intensity, an attempt was made to replace the well-known Hertzian diagrams of the field by a more complete series, representing the field for seven complete oscillations, and showing how the field for some twelve metres round the oscillator chosen, gradually falls to nearly $\frac{1}{50}$ of its maximum initial strength. These diagrams are entirely due to Miss Alice Lee, and involved a large expenditure of labour and time, which would, perhaps, not have been justified were any other graphic representation of a damped wave motion available.* These diagrams were originally intended for kinematograph representation, but that method of reproduction has not yet been found feasible.

(3.) The writers next deal with the type of waves propagated, their velocities and their phases. The following general conclusions are reached:—

(i) Three waves of electro-magnetic force may be considered as sent out from the oscillator, and not merely two as supposed by Hertz. These are:—

(*a*) A wave of purely transverse electric force.

(*b*) A wave of electric force parallel to the axis, briefly termed the wave of axial electric force.

(*c*) A wave of magnetic force.

The waves of axial electric and of magnetic force move outwards with the same velocity, which is, however, a function of the distance from the centre of the oscillator. The intensity of both forces for points on the same sphere varies as the cosine of the latitude, the polar axis being the axis of the oscillator.

The wave of transverse electric force is propagated with the same velocity at all equal distances from the centre of the oscillator, but this velocity differs from that of the two previous waves, the amplitude is independent of the latitude, being constant over any sphere. The velocity after the wave has reached a certain distance from the double point is always greater than that of the waves of magnetic and axial electric force. Its excess over the velocity of light tends to become three times the excess of the velocity of the magnetic wave over the velocity of light; both the excesses decreasing asymptotically.

(ii) The velocities of these waves undergo remarkable changes in the neighbourhood of the oscillator, but these changes extend to distances which are greater than those within which a great proportion of Hertz's interference experiments were made.

(iii) The point of zero phase for both transverse and axial electric waves does not coincide with the centre of the oscillator, so that these waves appear to start from spheres of small but finite radius round

* It must be remembered that a damped wave motion does not denote merely a factor $e^{-p_1 t}$ in the wave intensity, but a factor $e^{-p_1(t-ar)}$, which sensibly alters the shape of the lines of force propagated.

the oscillator. A fourth wave dealt with by Hertz, namely, the wave of magnetic induction, does not, as he supposes, start with zero phase from the origin, but with a finite phase. The wave in the equatorial plane, largely relied upon by Hertz for his interference experiments "of the first kind," is a compound of the waves of transverse and axial electric force, and has a much more complex series of velocity changes than Hertz appears to have realised.

(iv) The existence of the two electric force waves and the singular points or surfaces for the wave motion in the neighbourhood of the oscillator very possibly throw light on the difficulties which arise in Hertz's experiments. It would seem that such experiments should be made at distances greater than 6 to 7 $(\lambda/2\pi)$ from the centre of the oscillator, or, roughly, about a wave-length from the oscillator. In Hertz's case this amounts to about 10 metres—a distance at which Hertz rather terminated than started his interference experiments. Only at such a distance are the phase curves sensibly linear.

The authors are not unaware of the physical difficulties of experiment at great distances, and wish, therefore, to emphasise again the fact that they are dealing with a *theoretical* oscillator. It is, however, this type for which Hertz himself endeavoured to provide a mathematical investigation, and it is that investigation which, in the first instance, they have attempted to expand and modify.

January 26, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. G. J. Shaw-Lefevre, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Contributions to the Theory of Simultaneous Partial Differential Equations." By Dr. A. C. DIXON. Communicated by Professor G. J. ALLMAN, F.R.S.

II. "On the Structure and Affinities of Fossil Plants from the Palæozoic Rocks. III. On *Medullosa anglica*, a new Representative of the Cycadofilices." By Dr. D. H. SCOTT, F.R.S.

III. "On the Nature of Electro-capillary Phenomena. I. Their Relation to the Potential Differences between Solutions." By S. W. J. SMITH. Communicated by Professor RÜCKER, Sec.R.S.

"On the Structure and Affinities of Fossil Plants from the Palæozoic Rocks. III. On *Medullosa anglica*, a new Representative of the Cycadofilices." By D. H. SCOTT, M.A., Ph.D., F.R.S., Hon. Keeper of the Jodrell Laboratory, Royal Gardens, Kew. Received December 21, 1898—Read January 26, 1899.

(Abstract.)

The existence of a group of fossil plants, combining in their organisation certain characters of the Ferns and the Cycads, has been recognised, of late years, by several palæobotanists, as, for example, by the late Professor W. C. Williamson, Count Solms-Laubach, Mr. Seward, and the author. The convenient name, Cycadofilices, has recently been proposed by Professor Potonié to designate the group in question, which now includes several, somewhat heterogeneous, genera, among which *Lyginodendron*, *Heterangium*, and *Medullosa* may be mentioned.

Several species of the genus *Medullosa* (founded in 1832 by Cotta) have already been described, from the Permian and Upper Coal-measures of the Continent. They agree in the extraordinarily complex structure of the stem, which, as shown by Zeiller and Solms-Laubach, resembles in the ground plan of its organisation, that of a highly differentiated Fern, of the usual polystelic type, but with the addition of a zone of secondary wood and bast, sometimes reaching an immense thickness, developed around each stele. The mature stem thus acquired a Cycad-like character. The structure, however, has been extremely difficult to interpret owing to the comparative rarity and incomplete character of the specimens hitherto known.

No stem of a *Medullosa* has hitherto been recorded from this country, though specimens of *Myeloxylon*, now known to have been the petioles of *Medullosa*, are frequent in the calcareous nodules of the Lower Coal-measures.

The author has recently had the opportunity of investigating several excellent specimens of a new species of *Medullosa* from the Ganister Beds of Lancashire. These fossils are of special interest on several grounds; they are considerably more ancient than any members of the genus previously described, they are the first English specimens recorded, they are preserved in a more complete and perfect form than

any others at present known, and lastly, the greater simplicity of their structure causes the essential characters of the genus to stand out with greater clearness than in the more complex species. The specimens were discovered by Mr. G. Wild and Mr. J. Lomax, in material from the Hough Hill Colliery, Stalybridge. The sections have been cut, with the greatest skill and success, by Mr. Lomax, and are very numerous, about 100 sections, transverse and longitudinal, having been examined from one specimen alone.

The principal specimens are four in number, in addition to which other fragments have been included in the investigation. The species, which is very distinct from any form previously described, will be known as *Medullosa anglica*; a diagnosis is given below.

The most complete specimen of the stem has a mean diameter of rather more than 7 cm., including the adherent leaf-bases. The others do not appear to have been very different in dimensions.

The large leaf-bases, to judge from the most perfect specimens, almost completely clothed the surface of the stem. They were decurrent, and confluent with the stem for a vertical distance of 13 cm. or more, the diameter of the petiole, where it became free from the stem, being about 3 or 4 cm. The arrangement of the leaves was a spiral one, and in the only case where the phyllotaxis could be determined, the divergence proved to be 2/5.

In two of the specimens the external characters of the fossil are well shown. The outer surface of the long leaf-bases is marked by a conspicuous longitudinal striation, the ribs (which would not have been so prominent during life) representing the fibrous strands of the hypodermal tissue. The habit of the stem, clothed with the long, almost vertical, overlapping leaf-bases, may have been not unlike that of some of the tree-ferns, such as *Alsophila procera*.

The vascular system of the stem consists of three (or locally four) steles, anastomosing and dividing at long intervals. Each stele has an elongated, somewhat irregular, sectional form, and is composed of a central mass of primary wood, surrounded by a zone of secondary wood and phloëm. The primary wood, which is very well preserved, is made up of tracheides and conjunctive parenchyma, with the spiral elements (protoxylem) scattered near its outer margin. The secondary wood consists of radial series of tracheides and medullary rays; the secondary tracheides bear multiseriate bordered pits on their radial walls; most of the primary tracheides are pitted in the same way, but on all sides alike. In the neighbourhood of the protoxylem-groups the tracheides of the primary wood are spiral or scalariform. The phloëm is made up of elongated elements, presumably the sieve-tubes, forming a network, the meshes of which are occupied by the phloëm-rays.

Each stele of *Medullosa anglica* shows the closest agreement in structure with the single stele of a *Heterangium*, so that the stem of this

Medullosa might well be concisely described as a polystelic *Heterangium.*

The course of the leaf-trace bundles was followed very completely in consecutive series of transverse, and in longitudinal, sections. The leaf-traces leave the steles precisely in the same manner as in *Heterangium.* On becoming free the trace is a large concentric bundle, surrounded by its own zone of secondary wood and bast. As it passes obliquely upwards through the cortex, the trace loses its secondary tissues, and undergoes repeated division into a number of smaller bundles, each of which has collateral structure. These collateral strands have in all respects the same arrangement of their elements as the well known bundles of *Myeloxylon.*

The base of the leaf received a large number of bundles, consisting of the ultimate branches derived from the subdivision of several of the original leaf-traces. This distribution of the bundles is peculiar and unlike that in any known plants of Cycadean affinities.

In a few cases accessory vascular strands, of concentric structure, recalling the cortical bundles of a *Cycas,* were found to the outside of the normal stelar system.

The stem formed a well marked zone of internal periderm. In one specimen the whole of the outer cortex, with the leaf-bases, had been exfoliated, so that in this case the periderm formed the external surface.

The leaf-bases and petioles present in all respects, as regards hypoderma, vascular bundles, and gum-canals, the characters of the *Myeloxylon Landriotii* of Renault, which was evidently not a species, but a type of leaf-stalk common to various Medulloseæ. The petioles branched repeatedly, the finest ramifications of the rachis having a diameter of about 1 mm. only, but retaining in essentials the "*Myeloxylon*" structure. The leaf was thus a highly compound one; the structure of the leaflets associated with the rachis agrees well with that of the *Alethopteris* leaflets, figured by M. Renault.

The roots, never previously observed in any species of *Medullosa,* were of triarch structure, with abundant formation of secondary wood and bast, and an early development of internal periderm, by which the primary cortex was thrown off. Developmental stages show that the periderm originated in the pericycle. The roots, which branched freely, were borne on the stem in vertical series, between the bases of the leaves. They were attached to pedicels, through which the vascular tissues of the roots were continuous with those of the stem.

The author is indebted to Mr. J. Butterworth and Mr. G. Wild, for specimens which have thrown important light on the connection between root and stem.

The full paper concludes with a short historical *résumé,* and a discussion of affinities.

Medullosa anglica, in the structure of its stem, shows unmistakable

affinities with *Heterangium*, perhaps the most fern-like of the genera grouped under Cycadofilices. The new species is far simpler than any *Medullosa* hitherto described, for the steles are not only few but are uniform, showing no differentiation into a peripheral and a central system. The small central steles, called "Star-rings" in other Medulloseæ, are absent here. In these and other points the species agrees with the genus *Colpoxylon* of Brongniart, but as that genus is doubtfully distinct and its leaves are not known, it is not proposed to unite the English species with it.

In the structure of the petiole and of the leaf generally, *Medullosa anglica* is as highly organised as any of the Medulloseæ, and agrees closely with *M. Leuckarti*, the only other species in which the connection between leaf and stem has been at all satisfactorily proved.

In the structure of the petioles, and of the roots, in the secondary tissues, and in the secretory canals, which occur throughout the plant, there are clear points of agreement with Cycads, though the primary structure of the stem was that of a Fern. The affinities in the latter direction came out more clearly in *Medullosa anglica* than in any of the other species as at present known.

The habit of the leaves, if as appears likely, they were of the *Alethopteris* type, must have been fern-like, but that in itself, as the familiar example of *Stangeria* teaches, is as consistent with Cycadaceous as with Filicinean affinities.

While *Medullosa* thus combines, in a striking manner, the characters of Ferns and Cycads, the author is not disposed to regard it as having lain very near the direct line of descent of the latter group. It is more probable, as Count Solms-Laubach has suggested, that the Medulloseæ represent a divergent branch, which has left no descendants among existing vegetation.

Medullosa anglica, sp. nov.

Stem vertical, clothed by large, spirally arranged decurrent leaf-bases, perhaps cast off in old stems. External surface of leaf-bases longitudinally striate.

Vascular system of stem consisting of a few (usually three) uniform steles, somewhat elongated and lobed as seen in transverse section. Star-rings absent. Interior of each stele wholly occupied by primary wood.

Secondary wood and bast of moderate thickness, developed on all sides of the steles. Tracheides usually with bordered pits.

Leaf-traces concentric on leaving the steles, branching and becoming collateral in traversing the cortex.

Leaf-bases and petioles with the structure of *Myeloxylon Landriotii*, Ren.

Leaves highly compound.

Gum-canals abundant in the petioles and leaf-bases, and in the cortex, and around the steles of the stem.

Adventitious roots borne in vertical series, triarch, with secondary wood and bast, and periderm.

Stem with leaf-bases, about 7—8 cm. in mean diameter.

Petioles about 2·5—4 cm. in diameter at base, diminishing to about 1 mm. in the ultimate branches of the rachis.

Leaflets about 3 mm. wide.

Roots reaching 12 mm. in diameter.

Locality: Hough Hill Colliery, Stalybridge, Lancashire.

Horizon: Lower Coal-measures.

Found by Messrs. G. Wild and J. Lomax, 1892–98.

"On the Nature of Electro-capillary Phenomena. I. Their Relation to the Potential Differences between Solutions." By S. W. J. SMITH, M.A., formerly Coutts-Trotter Student of Trinity College, Cambridge; Demonstrator of Physics in the Royal College of Science, London. Communicated by Professor A. W. RÜCKER, Sec. R.S. Received January 5,—Read January 26, 1899.

(Abstract.)

1. The Lippmann-Helmholtz theory of the capillary electrometer contains two assumptions.

2. The first assumption would apply to any electrolytic cell. A deduction from it, which would apply to any cell having a large and a small electrode, is that the variation of the potential difference at the capillary electrode of an electrometer is the same as that of the applied electromotive force.

In order to trace the relation between surface tension and potential difference on the view that this first assumption is correct, it is necessary to eliminate the possible effect of depolarisation upon the form of the electro-capillary curve—*i.e.*, the curve which shows the relation between the surface tension and the applied electromotive force. A direct method of examining the depolarisation current is described and applied. An estimate of the magnitude of the depolarisation effect is given, and the circumstances under which the effect may become appreciable are discussed.

3. The second assumption of the Lippmann-Helmholtz theory, that the electro-capillary phenomena are controlled by a simple variation of the electrostatic surface energy, leads to two conclusions, each of which is beset with difficulties.

a. The form of the electro-capillary curve is remarkably dependent upon the nature and concentration of the electrolyte, and depolarisation is quite insufficient to account for the dependence.

b. The conclusion that the potential difference between the solution and the capillary electrode is zero when the surface tension has its maximum value, leads to the necessity for assuming large potential differences between certain solutions.

4. The hypothesis that the potential difference between equally concentrated solutions of potassium chloride and iodide is negligible possesses a high degree of probability. It has been shown by previous observers that if this hypothesis be true the points of maximum surface tension on the electro-capillary curves for the above solutions cannot have the significance which Helmholtz's theory gives them.

It is shown in the paper that the first hypothesis of the Lippmann-Helmholtz theory is in striking accord with this hypothesis concerning the potential difference between KCl and KI when the very definite "descending" branches of the electro-capillary curves are considered.

5. If both the hypotheses just mentioned be true, we get the result that the surface tension of mercury (for a certain range of potential differences) in two solutions is the same for a given potential difference between the mercury and the respective solutions, if the solutions are equally concentrated and possess the same kation.

6. An extension of this result shows that it is indifferent whether the kation be K, Na, or H.

7. The relation found for the KCl and KI curves can be extended to the other known cases in which the electrometer curves and liquid potential difference calculations seem to be contradictory, in such a way as to account for the apparent contradiction. Several of the cases are examined.

8. The results in 4, 5, and 6 would give a direct and accurate method of finding the potential differences between equally concentrated solutions, and could be extended to the case of solutions of different concentrations.

9. The probability that the electro-capillary curves are never completely free from influences other than electrostatic is shown by an examination of the relations between the curves for unequally concentrated solutions of the same salt.

10. In confirmation of results obtained by G. Meyer, in a slightly different way, it is shown that if the potential difference between KCl and KI is very small, the potential fall from a half normal solution of KI to a dropping electrode of the Paschen type is about a quarter of a volt greater than that from a half normal solution of KCl to the same electrode.

In the same way the potential fall from KI to mercury when the surface tension is a maximum is about a quarter of a volt greater than

that from KCl to mercury when the tension of the surface separating the solution from the mercury is a maximum.

These results follow from direct observations with dropping electrodes, and give further support to the view that the first assumption of the Lippmann-Helmholtz theory is true and that the second is not.

"The Influence of Removal of the Large Intestine and Increasing Quantities of Fat in the Diet on General Metabolism in Dogs." By VAUGHAN HARLEY, M.D., Professor of Pathological Chemistry, University College, London. Communicated by Professor VICTOR HORSLEY, F.R.S. Received July 25,—Read November 17, 1898.*

CONTENTS.

Introduction.

When I commenced my investigations into the functions of the large intestine by means of experimenting with isolated loops, into which milk was injected, and after some hours again collected, I found that the analysis yielded most unsatisfactory results. In consequence, in order to get over the difficulty, it seemed better to try the effect of the removal of the large intestine on nutrition. I believed that by comparing the analysis of the urine and fæces of dogs after removal of the large intestine with that of normal dogs on precisely similar diet, the effect of the absence of the large intestine would be sufficiently clearly demonstrated, and by this means its functions would be better understood.

* Received during recess and published in abstract in this volume at p. 77 supra.

In the earlier method of experimenting with loops of the large intestine, in some experiments the middle part of the large intestine was isolated, forming a Vella's fistula.

A given quantity of food was passed into the fistula, and after some hours it was again collected for analysis. By this means, however, I found it impossible to collect the entire quantity put into the fistula unless very large quantities of wash-water were used, which rendered the analysis fallacious. I do not therefore refer in this paper to the results thus obtained, but at the same time wish to draw attention to the fact that in these dogs the Vella's fistula was found in the intervals between experiments to fill up with *débris*. For example, in one case the middle (17 cm.) of the large intestine was separated, the upper and lower ends being sewn to the abdominal wound; the upper and lower part of the divided remnant of the intestine was connected together. The dog at first was fed on milk and gradually on a better diet, as, indeed, all the animals about to be described were treated. The operation was done in this case in March, 1894, and by May, after careful feeding, the dog had gained three pounds in weight, so that the absence of the large intestine had not hindered the animal from putting on flesh.

The Composition of the Excretion of a Loop of the Large Intestine.

In May the material that had collected in the loop was analysed, and again on another occasion in December of the next year when the dog was killed. The following are the results of these analyses :—

Analysis of *débris* collecting in loop of large intestine of dog.

	Qt.	Solids.	Water.	N.	Fat and cholesterin.	Ash.
	grams.	p. c.	p. c.	p. c.	p. c.	p. c.
May, 1894 ...	4·000	36·596	68·804	...	2·473	0·965
Dec., 1895 ...	3·2107	33·501	66·499	3·644	2·171	...

The analyses above given correspond very closely to those which have been found to occur in loops of the small intestine.

Hermann,* Ehrenthal,† Berenstein,‡ and Fr. Voit,§ in their examination of isolated loops of the small intestine, showed that they were apt to fill up with contents, which, microscopically and chemically, corresponded with the materials collected from the loop of large intestine in the above dog.

* L. Hermann, 'Pflüger's Archiv,' vol. 46, p. 93, 1890.

† W. Ehrenthal, *ibid.*, vol. 48, p. 74, 1891.

‡ M. Berenstein, *ibid.*, vol. 53, p. 52, 1892.

§ Fr. Voit, 'Zeit. f. Biol.,' vol. 29, p. 325, 1892.

In Voit's analysis the contents of the loop of the small intestine contained fat, nitrogen, and salts in a proportion very much corresponding to that found in the above dog, and he, together with others, considers this a normal excretion from the small intestine which goes in great part to form the fæces on a diet which is well absorbed.* The above result obtained in the large intestine is of importance, as it shows that a similar excretion occurs in the large intestine, as that already known to occur in the case of the small intestine, and this observation will explain in all probability the results obtained as regards cholesterin when the large intestine was removed (*vide* page 287).

With these preliminary remarks we can now turn to consider the present research, and before doing so, it is as well to describe briefly the operative procedure which was carried out.

Operative Procedure.

The dog, after being put under an anæsthetic, had its large intestine thoroughly washed out by means of an enema, and after abdominal section the bowel was divided just above the cæcum, and also as low down and as near the anus as possible. The lower part of the small intestine was then stitched into the rectum, care being taken to pre-

* v. Moraczewski has since the writing of the present article published a paper on contents of occluded portions of the intestine. His experiments are limited, however, to two dogs, in both of which cases after the loop had been isolated the animals were allowed to live for about a year, being well fed throughout.

In the first dog the loop consisted of part of the ilium, cæcum, and the commencement of the colon. The material collected at the end of a year from this loop was coloured, and the analysis was roughly as follows :—

Qt.	Solids.	Water.	Proteid.	Fat and cholesterin.	Ash.
grams.	p. c.	p. c.	p. c.	p. c.	p. c.
360	26	74	1	43	20

In the second dog only part of the ascending colon was isolated, so that we should here get the excretion from the large intestine, not contaminated by anything from the small. The contents in this case were colourless. Unfortunately the absolute quantity of analysis is not stated. However, he says it contained very little proteid or fat, and no cholesterin or lecithin, but principally only sodium carbonate.

From these results Moraczewski concludes that the secretion, or rather, as I should prefer, the excretion, from the different parts of the intestine differs, that from the small intestine containing colouring matter, fat, and cholesterin, while that from the large intestine is only sodium carbonate.

My own analysis of the contents of the large intestine, although naturally the quantity of material used was not so great and the period of collection much shorter, does not lead me to entirely agree with the above results. The fat was distinctly less than the quantity found in the small intestine of Moraczewski, but both fat and nitrogen were undoubtedly present, and the ash was extremely small ('Zeit. f. Physiol. Chem.,' 1898, vol. 25, p. 122).

serve a good blood supply, and as far as possible not to allow too much tension at the place of juncture, as otherwise the ligatures were apt to give way, and to be followed by a fatal peritonitis. The isolated portion of the large intestine, together with the cæcum, was in some cases merely closed at both ends so as to form a kind of Hermann's loop. It was found, however, that this was not satisfactory, for no amount of washing out the loop would remove all the bacteria therein contained, and the loop tended to fill with the normal excretion already described, and this, together with the bacteria present, led to rupture and fatal peritonitis. The loop was sometimes found to fill with a very watery fluid, so that in one case it was found distended with a dirty brown fluid even in spite of a slight rupture having already occurred. It was therefore decided that it would be more satisfactory to entirely remove the isolated portion of the large intestine.

Of the experiments about to be described in one case the large intestine was only partly removed—that is to say, slightly more than its middle third. In the other two dogs the large intestine was entirely removed together with the cæcum.

The dogs after the operation were then put on milk diet and the quantity was gradually increased with the addition of beef-tea and meat, until ordinary diet was able to be given; and the experiments on the metabolism were only carried out on dogs which had been on ordinary diet for some time, and had regained practically their normal weight.

The Diet Employed.

As far as the food used during the experiments, it was found most convenient to sterilise weighed out portions of minced meat, each quantity being sufficient for the day, and to this was added the given quantity of biscuit and fat as required. In all cases each meat, biscuit, or fat had been previously analysed, three separate samples being employed for the purpose, and the average of the three analyses were taken in calculating the nitrogen and fat of the diet.

The Methods of Analysis.

With regard to the method of analysis employed, and collection of material for analysis, the urine was collected by means of a catheter; at the same time the animals were kept in a cage, in case by any accident they should pass water by day or night, that also would be collected in the cage and added to that obtained by the catheter. As a matter of fact, dogs sufficiently often catheterised pass very little into the cage direct. The fæces were collected into the cage itself.

The nitrogen in the urine and fæces was in both cases analysed by

the method of Kjeldahl, and the sulphates after the method of Baumann.

As far as the fæces were concerned, in all cases they were examined with the ordinary precautions necessary in metabolism experiments, each daily quantity being separately examined, and the periods separated by means of charcoal. They were dried down over a water-bath, after adding dilute sulphuric acid, so as to avoid any escape of ammonia in the drying process. The drying was continued in a drying cupboard until they became a constant weight, and from this the quantity of water and solids was calculated. The dried residue was analysed for nitrogen and fat after the usual methods. Naturally in those cases where the fats were separately analysed, the portion of fæces taken for the analysis was not dried down with the sulphuric acid, but, on the other hand, extracted with alcohol, the alcohol extract being evaporated over a water-bath, and the total extracted with ether, and the fats, cholesterin, &c., separated, as I have already described in a former paper.*

As regards the carbohydrates, it was originally intended to estimate the quantity in all the experiments, but it was found in one normal dog, and in one dog in which the large intestine had been entirely removed, that of the carbohydrates given in a diet of biscuit and meat there was neither loss in the fæces in the normal, nor in the case of the complete absence of the large intestine; it was therefore considered unnecessary to repeat these experiments.

In order to investigate the effect of the removal of the large intestine on the general metabolism, it was necessary to examine in close detail the normal conditions of dogs fed on, roughly speaking, the same diet.

The Influence of Increasing Quantities of Fat in the Diet on the Metabolism of Normal Dogs.

For this purpose the quantity of proteid and carbohydrate throughout each research was kept constant, only the quantity of fat being increased during different periods, and the same experiments being repeated on dogs with either the partial or complete removal of the large intestine. In calculating the quantity of proteid or fat absorbed from the alimentary canal, no allowance is made for the quantity of nitrogen which is normally present in the fæces in a fasting animal.

C. Voit and Fr. Müller† have shown that even during fasting there is an elimination of fæces. For example, in a dog of 30 kilos. the average was about 2 grams of dried fæces daily, containing no less than 0·15 gram of nitrogen.

* Vaughan Harley, 'Roy. Soc. Proc.,' vol. 61, 1897.

† Fr. Müller, 'Zeit. f. Biol.,' vol. 20, p. 348, 1884.

Rieder* found that on feeding dogs with a nitrogen-free diet the fæces contained absolutely more nitrogen than during fasting, and often than that obtained on a pure meat diet. A dog on 500 grams of starch-meal gave 0·7 gram, with 700 grams of starch-meal it gave 0·8 gram of nitrogen in the fæces; while with 1500 grams of meat the fæces only contained 0·67 gram of nitrogen.

J. Tsuboi† has lately carried out the same researches with the greater accuracy of modern methods in Professor Voit's laboratory, and his results show that the nitrogen of the fæces on an absolutely nitrogen-free diet is greater than that during fasting, and the quantity of nitrogen increases with the quantity of food. In fact, the quantity of nitrogen in the fæces on a nitrogen-free diet may be as much as that found on the diet of meat rich in nitrogen.

It is thus seen that on such a diet the greater quantity of the nitrogen of the fæces must be regarded not as a nitrogen residue, but as the product of metabolism.

It has already been stated that the contents of the Hermann's loop are probably to be regarded as consisting of such metabolic products; and, as I have already shown in this paper, the large intestine behaves in exactly similar manner. Although by calculations on animals and man, one has a rough idea of the daily quantity of nitrogen and fat that ought to be eliminated during fasting, and although one can subtract that amount from the quantity found in the fæces, in the case where the large intestine was removed this could not be done, as we have no data showing the daily quantity of nitrogen or fat that is eliminated by the large intestine. It was therefore considered better in calculating absorption of proteids and fat, to neglect from the estimation the quantities probably excreted in fasting.

We will now consider the experimental details, and in the first instance I will refer to two normal dogs, which are taken for the purpose of comparison with those in which the large intestine was removed, and in which the quantity of fat ingested during various periods was steadily increased. These details are given in the following table (p. 261).

Dog 1.—In the preceding Table I we see the influence of the addition of fat to the diet on the absorption and metabolism in a normal dog.

In this table the sterilised meat was commenced eight days previous to the first analysis, the animal being on nitrogen equilibrium. The diet consisted of sterilised beef, biscuit, and small quantities of fat; water was given twice a day, and the dog was allowed to drink as much as she pleased each time.

(*a*) During the first period of four days the quantity of nitrogen in

* Rieder, *ibid.*, vol. 20, p. 382, 1884.

† J. Tsuboi, *ibid.*, vol. 35, p. 76, 1897.

Table I.

Dog 1.—The Influence of an Increasing Quantity of Fat to a Staple Proteid and Carbohydrate Diet on the general Metabolism and Absorption. Diet consisted of 100 grams of Sterilised Beef and 60 grams of Biscuits.

Day.	Weight.	Diet, total.		Urine.			Fæces.			Absorbed.	
		N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
(*a*) 8th....	4·59	4·82	12·04	125	1050	4·382	17·79	0·274	..	94·31	..
9th....	4·59	4·82	12·04	130	1062	4·810	18·65	0·299	0·710	93·79	94·10
10th....	4·59	4·82	12·04	120	1060	4·399	18·50	0·338	0·711	92·98	94·10
11th....	4·59	4·82	12·04	95	1058	4·236	19·51	0·494	0·778	89·75	93·54
Average ...	4·59	4·82	12·04	118	1058	4·236	18·61	0·351	0·733	92·71	93·91
(*b*) 15th....	4·59	4·82	32·04	95	1054	3·754	24·67	0·497	1·330	89·68	95·85
16th....	4·59	4·82	32·04	94	1054	3·625	20·53	0·439	1·141	90·89	96·44
17th....	4·59	4·82	32·04	80	1065	3·164	18·95	0·351	0·673	92·71	97·90
18th....	4·59	4·82	32·04	85	1067	3·755	17·52	0·359	0·741	92·55	97·72
Average....	4·59	4·82	32·04	89	1060	3·575	20·42	0·412	0·971	91·45	96·97
(*c*) 22nd ...	4·63	4·82	62·04	70	1060	3·235	19·62	0·393	0·876	91·84	98·59
23rd....	4·63	4·82	62·04	65	1065	3·340	28·26	0·620	1·583	87·13	97·45
24th....	4·63	4·82	62·04	60	1060	3·085	17·52	0·364	0·951	92·44	98·46
25th....	4·63	4·82	62·04	85	1057	3·789	25·41	0·500	1·644	89·62	97·35
Average....	4·63	4·82	62·04	70	1060	3·362	22·70	0·469	1·264	90·26	97·96

the urine remained almost constant; it rose somewhat on the ninth day, when a little more urine than usual was passed of a higher specific gravity. The average quantities are better discussed later on.

The fæces for the first two days contained rather less nitrogen than the next two, so that the percentage absorbed during the different days varied from 89·75 per cent. to 94·31 per cent. of the total nitrogen, as estimated by subtracting the quantity of nitrogen left in the fæces from the quantity known to be given in the diet. The fat in the diet was in this case 12·04 grams, and the fæces contained during three days from 0·710 to 0·773 gram, one day being unfortunately lost, so that the percentage absorption of fat fluctuated from 94·54 to 94·10.

(*b*) The diet on the twelfth day increased, so that the dog received 32·04 grams of fat, and charcoal was given on the fifteenth, and another period of four days analysed. During the four days the urine remained pretty well constant, being from 95 to 80 c.c. The nitrogen on the fifteenth, sixteenth, and eighteenth days was almost the same, 3·754 grams, but the seventeenth day it fell somewhat to 3·164 grams.

The quantity of fæces varied from 17·52 to 24·56 grams, and the nitrogen also varied, the daily quantity fluctuating between 0·351 and 0·497 gram, the fat showing again a greater fluctuation from 0·673 to 1·330 grams.

Thus the percentage absorption of proteids varied between a range of 89·68 and 92·71 per cent., while the fat varied between 95·85 and 97·90 per cent.

On the 19th another 30 grams of fat was added to the diet, so that the dog now received no less than 62·04 grams of fat per diem, as well as the original quantity of proteids and carbohydrates.

(*c*) On the 22nd the analyses were again begun, and carried on for four days. The quantity of urine varies from 60 to 85 c.c.; the specific gravity shows the same fluctuation.

The nitrogen in the urine had its lowest limit at 3·085 grams; its highest at 3·789 grams. The quantity of fæces varied from day to day between 17·52 and 28·26 grams, and there was a very marked difference in the quantity of nitrogen eliminated in the fæces during the eight days, being on one day as low as 0·364, while another day it reached as high as 0·620 gram.

The fat in the fæces varied from 0·876 to 1·644 grams per diem. This different quantity of nitrogen and fat on the different days caused the percentage of the nitrogen absorbed to vary from 87·13 to 92·44 per cent., while the percentage of fats varied from 97·35 to 98·59 per cent.

Dog 2.—In Table II, which gives the details of Experiment 2, there were two separate periods in which the quantity of fat given remained the same, the diet containing 8·00 grams of nitrogen and 15·20 grams of fat.

Table II.

Dog 2.—The Influence of an Increased Quantity of Fat to a Diet containing a Fixed Quantity of Proteid and Carbohydrate. The Diet consisting of 150 grams of Sterilised Beef and 100 grams of Biscuit.

Day.	Weight.	Diet, total.		Urine.			Fæces.			Absorbed.	
		N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
(*a*) 9th ..	6·41	8·00	15·20	130	1050	6·950	22·70	0·414	0·458	94·82	96·99
10th ..	6·41	8·00	15·20	130	1055	..	27·45	0·597	0·661	92·53	95·65
11th	6·41	8·00	15·20	100	1060	5·972	25·93	0·552	0·632	93·10	95·84
12th	6·41	8·00	15·20	115	1052	5·460	50·59	1·223	1·353	84·70	91·10
Average ..	6·41	8·00	15·20	119	1054	6·127	31·67	0·696	0·776	91·29	94·68
(*b*) 14th ..	6·41	8·00	15·20	90	1062	5·330	33·30	0·739	0·831	90·76	94·53
15th ..	6·41	8·00	15·20	85	1063	5·159	17·53	0·504	0·539	93·70	96·45
16th ..	6·41	8·00	15·20	115	1060	6·314	40·45	0·954	1·139	88·07	92·51
17th ..	6·40	8·00	15·20	143	1050	6·258	43·20	0·999	1·084	87·50	92·87
Average ..	6·41	8·00	15·20	108	1059	5·815	33·62	0·799	0·898	90·01	94·09
(*c*) 21st ..	6·47	8·00	65·19	93	1060	4·190	29·36	0·717	1·792	91·03	97·25
22nd ..	6·52	8·00	65·19	50	1062	3·115	34·41	0·830	2·074	89·62	96·82
23rd ..	6·63	8·00	65·19	80	1058	4·270	39·17	1·067	2·666	85·65	95·91
24th ..	6·75	8·00	65·19	..	..	..	42·09	0·988	2·466	87·64	96·22
Average ..	6 59	8·00	65·19	74	1061	3·858	36·26	0·901	2·249	88·73	96·55

(*a*) During the first four days the quantity of urine was rather high, from 100 to 130 c.c.; the nitrogen varied from 5·460 to 6·950 grams.

The fæces varied in the different days from 22·70 to 50·59 grams. It was indeed in consequence of a great increase of fæces on the twelfth day that it was thought advisable to give the dog another period of four days on the same diet.

The nitrogen varied, as one would expect, with the quantity of fæces, fluctuating from 0·414 to 1·223 grams, and the fat from 0·458 to 1·353 grams.

In this case the quantity of nitrogen, fat, and the daily quantity of fæces were separately analysed, so that the great rise on the twelfth day in both nitrogen and fat found eliminated in the fæces, was not due to an error of drying, but to the quantity absolutely found by analysis on that day.

(*b*) During the next period of four days, for some unexplained reason, the quantity of urine passed during the fourteenth and fifteenth days was markedly diminished, being only 90 and 85 c.c., while on the sixteenth and seventeenth it rose to 115 and 143 c.c. In spite of this variance in quantity the averages of the two periods, as will be later seen, are well within reasonable limits.

The nitrogen in the urine was, as one expected, increased during the sixteenth and seventeenth days, being no less than 6·314 and 6·258 gram, as against 5·159 and 5·530 grams on the fourteenth and fifteenth days, when the quantity of urine was low.

As far as the quantity of fæces was concerned, on the fourteenth day 33·30 grams were passed, while on the fifteenth day it dropped to only 17·53 grams; on the next days 40·45 to 43·10 grams were passed.

The quantity of nitrogen in the fæces varied from 0·504 to 0·999 gram, and the fat from 0·539 to 1·139, so that the absorption of nitrogen varied from 87·50 to 93·70 per cent., while the absorption of fat varied from 92·51 to 94·53 per cent.

On the eighteenth day the fat in the diet was increased to 65·19 grams, and the analysis commenced on the twenty-first day.

(*c*) During the three days on which it was analysed the quantity varied between 50 and 93 c.c., and the quantity of nitrogen from 3·115 to 4·270 grams.

In this stage of the experiment the quantity of fæces varied from 29·36 to 42·09 grams on the last day, and the nitrogen in the fæces being for the first day only 0·717, while on the twenty-third day it reached no less than 1·067 grams.

The fat was naturally very much increased by this large increase in the diet, being 1·792 grams on the twenty-first day, and on the twenty-third day 2·666 grams. The percentage of proteids, as indicated in

the fæces, varies on different days from 86·65 to 91·03, while the fat varies from 95·91 to 97·25.

It will be well to now compare the averages of the two preceding normal dogs before we discuss what occurs after the removal of the large intestine.

The normal averages are given in Table III.

Dog 1.—Table III during the three periods of the experiment (*a*), (*b*), (*c*) received the same quantity of nitrogen, 4·82 grams. During the period (*a*), 12·04 grams of fat were given; during period (*b*), 32·04 grams, and during period (*c*), 62·04 grams. As far as the weight was concerned, the increase of fat from 32 grams caused no real increase in the average weight. The further increase to 62 grams of fat caused an increase of weight from 4·59 to 4·63 kilos.

So far as the quantity of urine is concerned, the interesting fact is brought out that the increase of fat in the diet caused a progressive decrease in the quantity of urine from 118 to 89 c.c., and 70 c.c. respectively, and this corresponds with an increase of specific gravity in the different periods. The fat also caused a decrease of the quantity of nitrogen eliminated in the urine, the quantity falling from 4·457 to 3·575 grams, on increasing the fat from 12 to 32 grams, and still further to 3·362 grams, by increasing the fat ingested to 62 grams. The marked difference in the fall of nitrogen excreted during (*a*) and (*b*), when the fat was increased 20 grams, is due to the animal having been on rather a small quantity of nitrogen, although on nitrogen equilibrium. On the other hand, when later on the fat ingested was increased to 62 grams, there was but a small decrease in the elimination of nitrogen, because the animal was, comparatively speaking, fat. The quantity of fæces steadily increased as the fat was increased in the diet, rising from 18·61 to 20·42 and 22·70 grams during the periods (*a*), (*b*), (*c*).

The nitrogen in the fæces also steadily increased with the increase of fat in the diet. Thus, on the relatively poor fat diet, 0·351 gram of nitrogen was eliminated; but, on increasing the fat in the diet in spite of the fat containing no nitrogen, there was an increase in the quantity of nitrogen in the fæces to 0·412 gram, which still further increased to 0·469 gram when the fat in the diet was increased to 62·04 grams.

The fat in the fæces also increased from 0·733 to 0·971 gram, and to 1·264 grams as the fat was increased in the diet.

That the increase of fat in the diet should cause a decrease of nitrogen in the *urine* is what one naturally expects,* but that the increase in the fat ingested should cause an increase of nitrogen in the fæces is not what might have been *à priori* expected to occur. That

* A. Pugliese, 'Du Bois-Reymond's Archiv,' 1897, p. 473, shows that increasing the fat in a fixed diet causes a decrease in the nitrogen eliminated in the urine.

Table III.—Average of two Normal Dogs, on a Diet containing an Increasing Quantity of Fat.

No. of experiment.	Weight.	Duration of observation.	Diet.		Urine.			Fæces.			Absorbed.	
			N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	days.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
1 (*a*)	4·59	4	4·82	12·04	118	1058	4·457	18·61	0·351	0·733	92·71	93·91
(*b*)	4·59	4	4·82	32·04	89	1060	3·575	20·42	0·412	0·971	91·45	96·97
(*c*)	4·63	4	4·82	62·04	70	1060	3·362	22·70	0·469	1·264	90·26	97·96
2 (*a*)	6·41	4	8·00	15·20	119	1054	6·127	32·67	0·696	0·776	91·29	94·68
(*b*)	6·41	4	8·00	15·20	108	1059	5·815	33·62	0·799	0·898	90·01	94·09
(*c*)	6·59	4	8·00	65·19	74	1061	3·858	36·26	0·901	2·249	88·73	96·55

this is due to an increased excretion or secretion from the intestines seems unquestionable, as already suggested by Voit, Müller, &c.

When we turn to the question of absorption, we find that so far as the nitrogen is concerned an increase of fat causes a decrease in the percentage of absorption, while, as in period (*a*), with 12·04 grams of fat, 92·71 per cent. of the nitrogen was absorbed; on increasing the fat to 32·04 grams, 91·45 per cent. was absorbed; and on still further increasing it to 62·04 grams, 90·26 per cent. was absorbed.

The fat absorption, on the other hand, instead of decreasing with the increase of fat in the fæces, really increased, so that it would appear as if there was a greater absorption of fat on a diet rich in fat than on one poor in fat.

In period (*a*) only 93·91 per cent. of the total fat given was absorbed, as compared with 96·97 in (*b*) and 97·96 per cent. in (*c*). This apparent increased percentage of absorption of fat must be attributed to the quantity of fatty matter excreted normally from the intestine, so that when the fat in the diet is small this quantity alone is sufficient to alter the apparent percentage very markedly. On the other hand, when the quantity of fat in the diet is large the small quantity excreted by the intestines makes little difference in the quantity in the fæces, so that the percentage absorbed appears to be higher than what is really the case.

Continuing the discussion of the normal averages, we now come to dog 2 (*vide* Table III).

In the first two periods (*a*) and (*b*) the diet was exactly the same, the reason being that the individual days fluctuated so much that it was thought better to do two periods for a normal standard.

It is seen the quantity of urine was 119 and 108 c.c. per diem during the two periods (*a*) and (*b*). The same may be said of the nitrogen in the urine, which was 6·127 and 5·815 grams; the parallelism of the two periods is therefore close.

The fæces amounted to 31·67 and 33·62 grams, and the quantity of nitrogen in the fæces during these two periods was 0·696 and 0·799 gram, while the fat was 0·776 and 0·898 gram respectively.

The percentage of absorption of the proteids was 91·29 and 90·01 per cent., while the absorption of fat was 94·68 and 94·09 per cent.

On increasing the fat to 65·19 grams the animal increased in weight. The quantity of urine fell to only 74 c.c., while the specific gravity rose to 1061. The total quantity of nitrogen in the urine fell very markedly to 3·858 grams; the quantity of fæces rose to 36·26 grams, and contained 0·901 gram of nitrogen, so that the quantity of fæces and nitrogen had increased on increasing the fat in the diet in the same manner as in the case of the preceding dog.

The fat in the fæces increased to no less than 2·249 grams. In this case 88·73 per cent. of the nitrogen and 96·55 per cent. of the fat was

absorbed. Thus in this dog we have exactly the same results as in the preceding dog, and therefore we can take these two normal dogs as a standard for comparison with the results obtained after the removal of the large intestine.*

The Influence of Partial Removal of the Large Intestine on Metabolism.

We now come to consider the effect of removal of the large intestine on general metabolism under similar circumstances to what we have found in normal dogs on increasing the amount of fat contained in their diet. Before doing so, however, we will consider by way of preface a case of partial removal of the large intestine.

In this experiment the middle third of the large intestine was converted into a Vella's fistula, the cæcum being attached to the rectum, on March 20, 1894, at which time the dog (a female) weighed 12 lbs. In December of that year her weight had risen up to 15 lbs., and the experiments about to be described were not carried out till November, 1895.

During a part of the time, before the experiments were completed, the dog was fed up, and became so fat at one time as to be practically unable to walk down stairs. It was from the results which were then obtained in this case of the partial removal of the large intestine that the present research was entered into, although the original experiments had been intended to be an investigation into the absorption from the large intestine, using fistulæ for that purpose. The post mortem examination showed the fistulous part of the large intestine, which was over 17 cm. long, to be very much narrower than normal, and to contain *débris*, mostly impacted, in the region of the fistula. This was analysed as already stated.

In this case, from the ileo-cæcal valve to the sutured junction, the

* Wicke and Weiske, in some experiments on the influence of the addition of fats and starch to the diet on metabolism, experimented with sheep. These two observers found that increasing the quantity of fat in the diet caused an increasing quantity of fæces to be eliminated. At the same time, less nitrogen was excreted in the urine, so that the fat acted as a nitrogen sparer to the organism.

It is thus seen that these experiments, in which fat was added to the diet of herbivora, yielded the same results as are found in the above tables in the case of the carnivora, except in one small detail.

Wicke and Weiske did not find, according to their table, any marked decrease in the quantity of urine by increasing the quantity of fat in the diet, although their tables tend to show a decrease, while in the case of the carnivora we have got a very marked decrease.

We, therefore, can conclude that the normal dogs here shown are well capable of acting as standards of comparison to the results obtained after the removal of the large intestine, since the results compare very favourably with those found in the case of the herbivora ('Zeit. f. Physiol. Chem.,' 1895, vol. 21, p. 42; 1896, vol. 22, pp. 137 and 265).

distance was 5·5 cm., and from the junction to the outside of the anus was 10 cm. when stretched. The transverse measurement of the cæcal end of the large intestine was 5 cm., while the rectal end had dilated to no less than 10 cm. Thus a pouch had been formed above the anus which accounted for the changes in the fæces which took place during the time of observation. For when the animal first was put on solid diet, some three weeks after the operation, the fæces passed were always fluid, while, later on, they were more or less formed, and, as will be seen presently, became really of almost a normal consistence.

Dog 3.—Table IV. In this bitch three periods were analysed. In (*a*), first period, the diet contained 6·05 grams of nitrogen and 11·73 grams of fat; five consecutive days were analysed. The weight remained constant at 6·10 kilos. The daily quantity of urine passed varied from 210 to 135 c.c., while the specific gravity was between 1025 and 1044. The quantity of nitrogen excreted in the urine rose and fell between 5·244 and 5·390 grams; so that throughout these five days the nitrogen equilibrium was well kept up. The quantity of fæces daily eliminated was very irregular. The first day no less than 70·76 grams of fæces were passed, that is to say within twenty-four hours of the diet. Next day only 7·50 grams, and on the third day none at all were passed; on the other two days during which fæces were passed the amount was 65·77 and 57·72 grams. In consequence of this great difference in the quantity of fæces daily passed, the nitrogen contained in them was also variable, varying from no less than 0·176 to 1·488 grams.

As far as the fat analysis is concerned the average was obtained from the figures for only three days. At the same time as the average thus obtained came out roughly what one would expect, it would appear the dog in every way behaved as a normal dog. Naturally, owing to the absence of fæces on one day (the third), the percentage of absorption during the various days varied notably, both in the case of nitrogen and fat.

(*b*) The dog was now put on the same nitrogenous and carbohydrate diet, but the fat increased to 36·73 grams. The quantity of urine passed varied from 150 to 216 c.c., while the specific gravity fluctuated from 1028 to 1038, the nitrogen in the urine varying from 4·188 to 5·491 grams.

As far as the fæces were concerned during these four days, one day no fæces were passed; on the other days the quantity varied from 43·78 to 51·97 grams, the quantity of nitrogen, however, varying very little, viz., from 0·753 to 0·918 gram. The fat on the first day was very high, being 2·219 grams, while the lowest limit was 1·543 grams. In this stage also the absorption varied on the various days from 85 to 88 per cent. of the total nitrogen, and from 94 to 96 per cent. of the total fat. *During this period the dog steadily increased in weight.*

Table IV.—Dog 3. Partial Removal of the Large Intestine. The middle 17 cm. being removed and only 15 cm. being left. The lower 10 cm. dilating into a pouch after twenty-two months.

Increasing Quantities of Fat being added to a diet containing 100 grams of Sterilised Beef and 100 grams of Biscuit. Figures in ( ) not complete average, owing to one analysis in each case being lost.

Day.	Weight.	Diet.		Urine.			Fæces.			Absorbed.	
		N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
(*a*) 12	6·18	6·05	11·73	210	1025	5·930	70·76	1·146	..	81·06	
13	6·18	6·05	11·73	200	1030	5·601	7·50	0·176	0·267	97·07	97·72
14	6·18	6·05	11·73	180	1032	5·873	..	no fæces			
15	6·18	6·05	11·73	135	1044	5·333	63·77	1·150	2·211	80·99	81·15
16	6·18	6·05	11·73	135	1044	5·244	57·72	1·488	2·390	75·40	79·62
Average	6·18	6·05	11·73	172	1035	5·596	39·95	0·792	(1·217)	86·91	(86)
(*b*) 19	6·19	6·05	36·73	160	1031	5·491	49·80	0·786	2·219	87·01	93·96
20	6·20	6·05	36·73	216	1028	5·251		no fæces			
21	6·26	6·05	36·73	150	1028	4·188	51·97	0·918	1·985	84·83	94·60
22	6·26	6·05	36·73	150	1038	5·032	43·78	0·753	1·543	87·55	95·80
Average	6·23	6·05	36·73	169	1031	4·991	36·39	0·614	1·437	89·85	96·09
(*c*) 30	6·68	6·05	51·73	120	1048	4·307	54·38	0·918	3·050	84·83	94·10
31	6·75	6·05	51·73	112	1045	4·763	24·60	0·351	0·762	94·20	98·53
32	6·75	6·05	51·73	130	1040	4·971	39·70	0·779	..	87·12	
33	6·75	6·05	51·73	100	1052	..	19·69	0·322	0·553	94·68	98·93
34	6·75	6·05	51·73	96	1052	..	53·18	0·752	1·602	87·14	96·90
Average	6·74	6·05	51·73	112	1048	(4·680)	38·31	0·624	1·492	89·69	(97)

(c) Another period of five days was next investigated, when the diet was increased still more to 51·73 grams of fat. The quantity of urine now passed varied from 96 to 130 c.c., with a specific gravity of 1040 to 1052. The nitrogen was only analysed in the urine during three days out of the five in consequence of an unfortunate accident. It varied from 4·307 to 4·971 grams, and remained pretty constant during this period. The dog daily passed his fæces, and the quantity in consequence appears smaller than in the preceding cases, varying from 19·69 to 54·38 grams; at the same time the quantity of nitrogen contained in the fæces varied from 0·322 to 0·918 gram. On the third day of this period the fat in the fæces was lost; on the other days the quantity varied from 3·050 to 1·602 grams.

With this we get a varied absorption of proteids, the quantity vary ing from 84·83 to 94·68, while as far as the fat is concerned it varied from 94·10 to 98·93 per cent.

Having considered this table in detail, we can now consider the average of the three periods (a), (b), (c) in this case, where partial removal of the large intestine had been carried out.

It was found in this partial removal, even after shrinkage of the part isolated, that over a half of the total length of the large intestine had been removed. The dilatation of the rectum accounts for the retention of fæces on some days so as to cause constipation.

From Table V the average of the three periods, the addition of an increasing quantity of fat being added to a fixed nitrogen diet, is seen to cause a decrease in the quantity of urine, the amount falling from 172 to 169 c.c., and then to 112 c.c. The specific gravity did not quite coincide, as it did not rise steadily in the three periods.

As far as the nitrogen in the urine is concerned, we see also that as the fat was increased, so the quantity of nitrogen in the urine fell from 5·596 to 4·991 grams and 4·680 grams. So that as in the normal dog the nitrogen sparing properties of the fat are well brought out.

The quantity of the fæces for all practical purposes is not much influenced by increasing the quantity of fat, and certainly not in the degree which would seem to occur in the two normal dogs, in both of which, on the fat being increased in the diet, the quantity of fæces were augmented. The variation may be in part explained by the constipation which occurred in periods (a) and (b).

The nitrogen of the fæces in this dog, if anything, was decreased in quantity by increasing the fat, for whereas during the first five days, period (a), 0·792 gram of nitrogen was daily eliminated in the fæces, on the diet being increased to 36·73 grams of fat during the four days of period (b), the nitrogen was increased to 0·614 gram, and during period (c), when no less than 51·73 grams of fat were being taken, the nitrogen amounted to 0·624 gram.

With regard to the fat in the fæces, the table shows that the quan-

Table V.—Average of Partial Removal of Large Intestine on a Diet containing an Increased Quantity of Fat. Figures in ( ) not complete average, owing to an analysis in each case being lost.

No. of experiment.	Weight.	Duration of observation.	Diet.		Urine.			Fæces.			Absorbed.	
			N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	days.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
3 (*a*)	6·18	5	6·05	11·73	172	1035	5·596	39·95	0·792	(1·217)	86·91	(86)
(*b*)	6·23	4	6·05	36·73	169	1031	4·991	36·39	0·614	1·437	89·85	96·09
(*c*)	6·75	5	6·05	51·73	112	1048	(4·680)	38·31	0·624	(1·492)	89·69	(97)

tity, 1·217 grams, in the first period was increased to 1·437 grams, when the diet contained 36·73 grams of fat, and on increasing the diet to 51·73 grams of fat, it rose to 1·492 grams.

When we compare this to what we find in normal dogs, we see the total quantity of fat daily eliminated was really higher than what occurred in the normal dogs, for in dog 1, on practically the same diet, the quantity of the three periods (*a*), (*b*), (*c*) was 0·733, 0·971, and 0·264 gram.

We now turn to the table of absorption. On the diet containing 11·73 grams of fat, the nitrogen absorption was 86·91 per cent. as against in the normal dog 92·71 per cent. On increasing the fat to 36·73 grams the nitrogen absorption rose to 89·85 per cent. On increasing the amount of fat in the diet to 51·73 grams it fell slightly to 89·69 per cent., while the corresponding absorption in the normal dog was 91·25 and 90·26 per cent. From this we see that on comparing the two animals together, we have a slight decrease in the percentage of absorption of proteids from the alimentary tract caused by partial removal of the large intestine. It also follows that contrary to what one finds in normal dogs, namely, that on increasing the fat in the diet there is an apparently decreased absorption, one finds in the dog from which the larger part of the large gut has been removed, on the other hand, an increased absorption of nitrogen.

Turning now to the fat, the average for which the periods (*a*) and (*c*) is not complete, for the whole period analysed it appears that 86* per cent. of the fat is absorbed as against 93 per cent. in the normal dog, while when the fat is increased 96·09 per cent. is absorbed in period (*b*) and 97 per cent. in period (*c*). So that while it is seen that on increasing the fat the percentage absorption is increased as in the normal dog, at the same time it must be noted that with a small fat diet the absorption appears to have been less than normal, although this may not be quite correct, since the average, as already stated, is not for the total period.

On the increased fat in the diet of stage (*b*) the percentage absorption of fat practically corresponds to what one finds in the normal dog, in this case being 96·09 per cent.; and again when the fat was increased to 51·73 grams, the absorption rose to 97 per cent., the corresponding figure in the normal dog with 62·04 grams of fat being 97·96 per cent.

It may therefore be considered that after partial removal of the large intestine the influence on general metabolism, as indicated by the urine, is very little; that increasing the fat in the diet causes, as in the normal dog, a steady decrease in the quantity of urine, and also causes a sparing of nitrogen to the body, and therefore decrease of nitrogen in the urine.

* This figure is probably too low. See Table V.

Increasing the fat in the diet of this dog did not increase the quantity of fæces excreted, and still further did not increase the quantity of nitrogen contained in the fæces as occurred in the normal dogs. At the same time it increased the quantity of fat found in the fæces.

The percentage of nitrogen absorbed under the same circumstances was decreased, but instead of decreasing with the increase of fat as in normal dogs, it practically remains the same, or if anything increases. In fact, the percentage absorption of fat would appear to be exactly the same as in the normal dogs.

The Influence of Complete Removal of the Large Intestine on Metabolism.

Having now finished the consideration of the dog in which the large intestine was partially removed, we will next examine the cases of the two dogs in which the whole of the large intestine was entirely removed together with the cæcum. In these instances the small intestine, just above its junction with the cæcum, was sewn into the rectum as close to the anus as possible; in fact, this was in every experiment found by post mortem to be under 6 cm.

The difficulty in the after-treatment consists in the straining movements of the anus being apt to tear the sutures.

During the first few days after the operation the animal was fed on milk and beef-tea; later on, the diet was slowly increased, and finally the animal was able to take the normal diet. For some reason or other they were unable to take such large quantities of fat as normal dogs, for when the fat was increased to a certain amount, they either had a severe diarrhœa or refused their food altogether, so that it was not possible to obtain the effect of the marked increase of fat on the composition of the fæces so clearly as in the normal dogs.

Dog 4.—In dog 4 the large intestine was entirely removed one month previous to the beginning of the metabolism observations, during which time the diet had been steadily increased as already described, and the animal had begun to feed well on its mixed diet. When its body weight had reached 4 kilos. the dog was then placed on the meat and biscuit diet, and the employment of sterilised meat was begun eight days before the commencement of the analysis.

The general results are included in Table VI.

In this case (dog 4) four periods were investigated, two periods in which the amount of fat given was 9·71 grams, the nitrogen in the proteid amounting to 6·80 grams; in the next two periods the fat was increased to 29·71 grams, while the meat and biscuit diet remained the same. In this case it was found impossible to increase the fat still more, as the dog then always refused his food.

(*a*) During the first period the weight remained constant, and the *quantity of water* passed fluctuated from 110 to 275 c.c., with a specific

Table VI.—Dog 4. The Influence of an Increasing Quantity of Fat to a Staple Proteid and Carbohydrate Diet on the general Metabolism and Absorption after Complete Removal of the Large Intestine one month previous to the commencement of Experiment. The diet consisted of 100 grams of Sterilised Meat and 100 grams of Biscuit.

Day.	Weight.	Diet.		Urine.			Fæces.			Absorbed	
		N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
(*a*) 8	4·05	6·80	9·71	110	1042	4·427	56·70	0·907	0·548	86·67	94·36
9	4·05	6·80	9·71	170	1028	4·815	97·82	1·383	0·734	79·68	92·44
10	4·05	6·80	9·71	210	1014	4·120	76·27	1·022	0·777	84·98	92·00
11	4·05	6·80	9·71	275	1016	4·418	71·42	0·945	1·049	86·11	89·20
Average	4·05	6·80	9·71	191	1025	4·445	75·55	1·064	0·777	84·36	92·00
(*b*) 15	4·05	6·80	9·71	395	1012	4·405	79·60	1·258	0·632	81·51	93·49
16	4·05	6·80	9·71	309	1013	4·290	69·43	1·044	0·713	84·66	92·66
17	4·11	6·80	9·71	330	1018	4·427	81·12	1·036	0·560	84·77	94·23
18	4·11	6·80	9·71	..	..	..	68·67	0·987	0·513	85·49	94·72
Average	4·08	6·80	9·71	341	1014	4·374	74·41	1·081	0·605	84·11	93·77
(*c*) 22	4·17	6·80	29·71	121	1029	3·010	82·56	1·162	0·805	82·92	97·20
23	4·17	6·80	29·71	200	1018	3·282	84·64	1·144	0·850	83·19	97·14
24	4·17	6·80	29·71	240	1014	3·344	83·09	1·005	0·699	85·23	97·65
25	4·17	6·80	29·71	265	1015	3·337	84·82	1·042	0·721	84·69	97·58
Average	4·17	6·80	29·71	207	1019	3·243	83·78	1·088	0·769	84·01	97·41
(*d*) 29	4·24	6·80	29·71	52	1034	1·330	82·90	1·180	1·248	82·66	95·80
30	4·30	6·80	29·71	300	1018	3·220	81·53	1·214	0·660	82·16	97·78
31	4·31	6·80	29·71	190	1023	3·960	85·10	0·918	0·710	86·51	97·61
32	4·31	6·80	29·71	240	1018	3·620	84·45	1·079	..	85·90	
Average	4·29	6·80	29·71	198	1023	2·965	83·49	1·093	0·873	84·14	97·06

gravity varying from 1016 to 1042. In spite of the great fluctuation in the quantity of water, the quantity of nitrogen daily eliminated in the urine varied only from 4·815 to 4·120 grams, so that there was no great difference per diem.

The quantity of fæces which were passed daily varied from 56·70 to 97·82 grams, and the nitrogen contained in the fæces fluctuated from 0·907 to 1·383 grams. The fat in the fæces during the eighth, ninth, and tenth days varied from 0·548 to 0·777 gram, while on the eleventh day, from some unknown reason, in spite of the fæces not having increased markedly in quantity, the quantity of fat contained therein was 1·041 grams. The absorption of nitrogen, in consequence of the varied quantities passed on the different days, fluctuated somewhat considerably, consequently another period of analysis was done on exactly the same diet for another period of four days, the diet in the interval having been kept the same.

(*b*) In this second stage of the experiment a part of the urine was unfortunately lost on the eighteenth day. The daily quantity varied from 309 to 395 c.c., with a specific gravity of 1012 to 1018. That is to say, the quantity was very much increased above that found during the previous four days, and the specific gravity very much lower. The quantity of nitrogen in the urine during these three days varied from 4·290 to 4·427 grams, so that that factor tallied very much with that found on the previous day.

The daily quantities of fæces were also more equal than on the previous four days, as they varied from 68·67 to 81·12 grams, the nitrogen varying from 1·258 to 0·987 gram; the daily fat in this case being fairly equal, and showing no marked rise as in the preceding period, varying from 0·713 to 0·513 gram.

(*c*) During the next (the third) period the quantity of fat was increased to 29·71 grams on the nineteenth day; and the analysis began on the twenty-second. The weight went up from 4·11 to 4·17 kilos. The quantity of urine passed fluctuated from 121 to 265 c.c., with a specific gravity varying from 1014 to 1029. The nitrogen in the urine decreased in amount, fluctuating from 3·010 to 3·344 grams. The quantities of fæces observed daily were very nearly equal, varying from 82·56 to 84·82 grams, the nitrogen in the fæces remaining very constant, the fluctuations being only between 1·005 and 1·162 grams, and the fat also remained very constant, varying from 0·699 to 0·850 gram.

(*d*) In the final period (the fourth), the food being in the interval kept exactly the same, the animal went up somewhat in weight, rising to 4·31 kilos. The quantity of urine passed varied from 52 to 240 c.c.; but the small quantity of urine passed on the twenty-ninth day is in all probability an error, as the specific gravity on that occasion was practically the same as on other days. Leaving this day's amount

out of account, the quantity of urine collected on the other three days was 310, 190, and 240 c.c. respectively, with a specific gravity varying from 1018 to 1023. The nitrogen observed remained throughout the period very constant, varying from 3·220 to 3·960 grams. The quantity of fæces again here, as in the preceding period, was fairly equal in amount, the quantity varying from 81·53 to 85·10 grams. The quantity of nitrogen in the fæces during these four days varied from 0·918 to 1·214 grams; the smallest quantity was found on the day on which the largest quantity of fæces was passed, that is to say, on the thirty-first day. The fat was only analysed on three out of the four days.

Before discussing the average results of these four periods of analysis it would be well to discuss the facts of the next experiment, in which the large intestine was also entirely removed, so that the two averages may be compared together.

In this dog only two periods were investigated, to try to fill up the gap in the observations just described, which were incomplete owing to the impossibility of increasing the fat in the dietary in sufficient degree.

Dog 5.—The first diet was started ten days before the analysis was commenced. During the first period of five days the animal received 6·26 grams of nitrogen in his diet, and 11·55 grams of fat. The quantity of urine during these five days varied from 75 to 120 c.c., the specific gravity varying from 1028 to 1052, the nitrogen, however, remaining pretty constant throughout, only varying from 4·110 to 4·508 grams. The quantity of fæces steadily increased in amount during the five days, rising from 46·93 to 94·20 grams, the nitrogen thereof also rising as the fæces increased, although it rose still more on the fourteenth day, when the fæces were not quite so high as on the thirteenth; it varied from 1·194 to 0·796 gram.

The quantity of fat was only analysed on four of these days, and varied from 0·380 to 0·618 gram.

During the next period of four days the fat in the diet was increased to 41·55 grams; however, during the last two days (twenty-fifth and twenty-sixth) the animal lost his appetite, although from the fifteenth to the twenty-fifth he had steadily kept up his diet and gone up in weight, and even stopping the fat for an interval and trying to administer it again was unsuccessful. Consequently, although the four days were analysed, only the first two days can be taken as an average on this diet.

We see on looking at the table that the quantity of fæces daily passed varied very considerably, and therefore the results obtained during the four days cannot be considered definite, but I have included the facts as an addition to the preceding observations.

On the increased fat diet it will be seen that the quantity of urine

Table VII.—Dog 5. The Influence of an Increasing Quantity of Fat to a Staple Proteid and Carbohydrate Diet on the general Metabolism and Absorption after Complete Removal of the Large Intestine two months previous to the commencement of experiment.

The diet consisted of 100 grams of Sterilised Meat and 100 grams of Biscuit.

Day.	Weight.	Diet.		Urine.			Fæces.			Absorbed.	
		N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Fat.
	kilos.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
10	6·30	6·26	11·55	90	1052	4·508	46·93	0·796	0·618	87·29	94·65
11	6·30	6·26	11·55	86	1042	4·110	59·30	0·974	0·380	84·45	96·71
12	6·30	6·26	11·55	120	1028	4·461	83·80	1·077	0·599	82·80	94·81
13	6·30	6·26	11·55	96	1048	4·343	92·20	1·114	0·614	82·21	94·68
14	6·30	6·26	11·55	75	1052	4·459	86·55	1·194	..	80·93	
Average	6·30	6·26	11·55	93	1044	4·376	74·16	1·031	(0·553)	83·54	(95)
23	6·63	6·26	41·55	96	1026	2·422	64·55	0·743	0·930	88·13	97·75
24	6·62	6·26	41·55	85	1030	2·408	54·20	0·468	0·701	92·53	98·31
Average	6·58	6·26	41·55	91	1028	2·415	59·38	0·606	0·819	90·33	98·03
25	6·53	6·26	41·55	82	1030	2·748	30·38	0·398	0·507*		
26	6·30	6·26	41·55	100	1022	2·406	14·77†				

* Not taken all food.

† Not taken all food. Urine, on standing, deposited indigo.

during the first two days was 96 c.c. and 85 c.c., with a specific gravity of 1026 and 1030. The quantity of nitrogen in the urine fell very considerably to 2·422 and 2·408 grams, while the quantity of fæces also was not very high, being 64·55 to 54·20 grams. The nitrogen in the fæces was lower than in the former period, being only 0·743 and 0·468 gram. The quantity of fat found was somewhat greater, being 0·936 to 0·701 gram.

We now come to consider Table VIII, in which are given the averages of the results obtained from these last two dogs, in which the large intestine had been entirely removed.

In dog 4 the quantity of urine during the two periods in which 9·71 grams of fat were given varied very considerably, being 191 and 341 c.c. respectively; and the specific gravity also varied, being 1025 and 1014. At the same time the quantity of nitrogen in the urine during these two periods remained pretty much the same, being 4·445 and 4·374 grams. The nitrogen in the fæces also remained constant, being 1·064 and 1·081 grams, the fat being 0·777 and 0·605 gram.

We see, comparing these with the normal dog on the same quantity of fat, that the quantity of fæces is very much larger, in fact nearly double the quantity found in the normal dog on practically the same diet, and at the same time the nitrogen in the fæces is increased, being more than double, or at any rate double the quantity in a normal dog, whereas the fat in the fæces is practically the same as in a normal dog on a diet containing this amount of fat.

As far as the absorption of proteids is concerned it is 84·36 and 84·11 per cent., while in a normal dog we find that on this diet roughly 92·71 and 91·29 per cent. was absorbed, and in the dog in which the large intestine was partially removed 86·91 and 89·85 per cent. was absorbed. We may, therefore, take it that this dog shows a still greater decrease in the absorption of nitrogen than after the partial removal of the large intestine, and that the absorption of nitrogen is very much influenced by the absence of the large intestine.

During the next two periods 29·71 grams of fat were taken, and the animals increased in weight during these two periods, also the quantity of urine varied very considerably, being 207 and 198 c.c., with a specific gravity of 1019 and 1023.

The quantity of nitrogen excreted in these two periods was 3·243 and 2·965 grams, so that the increase of fat caused a decrease in the quantity of nitrogen in the urine; that is to say, a proteid sparing influence, the same as in the normal dog. As far as the urine is concerned, and comparing these two periods with the former two periods, it would seem as if the quantity of water absorbed is decreased by the increased quantity of fat. At the same time this is not at all so well brought out as in the case of the normal dogs, or even in the dog with partial removal of the large intestine.

Table VIII.—Average, after Total Removal of Large Intestine, of Metabolism in Two Dogs on an Increasing Quantity of Fat in the Diet.

Figures in ( ) not complete averages.

No. of experiment.	Weight.	Duration of observation.	Diet.		Urine.			Fæces.			Absorbed.	
			N.	Fat.	Quantity.	Sp. gr.	N.	Quantity.	N.	Fat.	N.	Ft.
	kilos.	days.	grams.	grams.	c.c.		grams.	grams.	grams.	grams.	per cent.	per cent.
4 (*a*)	4·05	4	6·80	9·71	191	1025	4·445	75·55	1·064	0·777	84·36	92·00
(*b*)	4·08	4	6·80	9·71	341	1014	4·374	74·71	1·081	0·605	84·11	93·77
(*c*)	4·17	4	6·80	29·71	207	1019	3·243	83·78	1·088	0·769	84·01	97·41
(*d*)	4·29	4	6·80	29·71	198	1023	2·965	83·49	1·098	0·873	84·14	97·06
5 (*a*)	6·30	5	6·26	11·55	93	1044	4·376	71·16	1·031	(0·553)	83·54	(95)
(*b*)	6·58	2	6·26	41·55	91	1028	2·415	59·38	0·606	0·819	90·33	98·03

The quantity of fæces during the two periods is 83·78 and 83·49 grams, so that the increase of fat has undoubtedly caused in this dog an increased quantity of fæces. The nitrogen during these two periods was 1·098 and 1·088 grams, so that the increase in the quantity of fat in the diet has caused a slight increase in the quantity of nitrogen in the fæces, the same as we find in the normal dogs. The quantity of fat in the two periods was 0·769 and 0·873 gram, that is to say, a slight tendency to an increase, through the increased quantity of fat in the diet.

We thus see that the dog after total removal of the large intestine shows a marked increase in the quantity of nitrogen in the fæces; at the same time, in spite of this increase, it can be still more increased by increasing the fat given in the diet. As far as the fat is concerned the quantity of fat in the normal dog's fæces, as compared with that after removal of the large intestine, is practically the same. It is thus well borne out that as far as the fats are concerned they are well absorbed by the small intestine, the large intestine having no influence whatever on their absorption.

On the other hand, as far as the proteids are concerned, they are not so well absorbed—at least 10 per cent. less—but there is an increase in the quantity of proteid or nitrogen in the fæces of the dogs in which the large intestine has been removed, and that this increase is slightly augmented by increasing the quantity of the fat in the diet, as in normal dogs; at the same time the decrease in absorption, as thus indicated in normal dogs, is not so marked, if at all marked, after removal of the large intestine.

Having considered the general metabolism, and having seen how the quantity of fæces is increased in dogs in which the large intestine has been removed, we now come to consider the fæces more carefully as regards their general appearance and the quantity of water.

The Influence of Diet on the Daily Quantity and the Amount of Water in the Fæces when the Large Intestine is Removed.

Immediately after the operation, the dogs for the first few days took practically no food, and then were put on a milk diet, which consequently caused the light motions so typical of this diet. When the diet was slowly converted into that of biscuit and meat, on which a normal dog would be passing well-formed fæces, the dog continued to pass a semi-fluid motion. In the case of the partial removal of the large intestine this was well marked, so that the dog for the first few weeks suffered from more or less diarrhœa. Before death, however, throughout the time in which metabolism experiments were being done, the dog passed fæces almost of normal consistence, and, as is seen by the post mortem, this is to be explained by the sack-like enlargement of the rectum.

In normal dog 1, Table IX, are placed together the analyses of the fæces, as influenced by the increase of fat in the diet. The fat is seen here to cause not only an increase in the quantity of fæces, but also an increase in the total quantity of water excreted in the fæces. While we saw that increasing the fat in the normal dog's diet caused a decrease in the quantity of urine passed, we find here that it causes an increase in the quantity of water eliminated by the fæces, being on the first diet 12·79 grams, then 13·79 grams, and on the diet rich in fat no less than 14·32 grams. The percentage of water in the total fæces, however, decreases with the increased quantity of fat, as it falls from an average of 70·98 to 67·85 and 67·87 per cent. Thus the increased quantity of water eliminated by the fæces is due to the increased quantity of fæces, and not to an increased percentage of water present.

Table IX.—Normal Dog 1. Showing the Influence of an Increasing Quantity of Fat in the Diet on the Quantity of Fæces and the Water contained in them.

Day.	Diet.		Fæces.		
	N.	Fat.	Quantity.	Water. Total.	Water. Per cent.
	grams.	grams.	grams.	grams.	
8	4·817	12·043	17·79	13·39	72·25
9	4·817	12·043	18·65	16·11	75·63
10	4·817	12·043	18·50	13·06	76·64
11	4·817	12·043	19·51	12·59	59·39
Average	4·817	12·043	18·61	12·79	70·98
15	4·817	32·043	24·67	16·53	66·99
16	4·817	32·043	20·53	13·71	66·79
17	4·817	32·043	18·95	1·284	67·73
18	4·817	32·043	17·52	12·07	70·30
Average	4·817	32·043	20·42	13·79	67·95
22	4·817	62·043	19·62	13·42	68·40
23	4·817	62·043	28·26	18·59	65·79
24	4·817	62·043	17·52	11·94	68·15
25	4·817	62·043	25·41	13·31	68·13
Average	4·817	62·043	22·70	14·32	67·87

In dog 2, Table X, we have two periods in which the quantity of fat remained constant. As far as the quantity of fæces was concerned we found during these two periods the quantity pretty nearly the

same, at the same time the quantity of water differs somewhat considerably.

Table X.—Normal Dog 2. Showing the Influence of an Increasing Quantity of Fat in the Diet on the Quantity of Fæces and Water of the Fæces.

Day.	Diet.		Fæces.		
	N.	Fat.	Quantity.	Water.	
				Total.	Per cent.
	grams.	grams.	grams.	grams.	
9	7·995	15·200	22·70	16·17	71·22
10	7·995	15·200	27·45	18·03	65·68
11	7·995	15·200	25·93	14·08	54·30
12	7·995	15·200	50·59	31·30	61·85
Average	7·995	15·200	31·67	19·90	63·26
14	7·995	15·200	33·30	22·42	67·31
15	7·995	15·200	17·53	10·29	58·69
16	7·995	15·200	40·45	26·63	65·82
17	7·995	15·200	43·20	28·77	66·59
Average	7·995	15·200	33·62	22·03	64·60
21	7·995	65·190	29·36	17·95	61·14
22	7·995	65·190	34·41	21·20	61·61
23	7·995	65·190	39·17	29·19	55·65
24	7·995	65·190	42·09	26·32	62·68
Average	7·995	65·190	36·26	23·67	60·52

The percentage of water in this dog was less than the percentage on the corresponding diet in the first normal dog, being only 63·26 and 64·60 per cent. On increasing the fat in the diet, the quantity of fæces, as in the former normal dog, increased, and the total water eliminated also increased. The percentage of water, however, decreased, so that though the increase of fat in the diet caused in this dog, as in the former, an increased elimination of water in the fæces, this was due to an increased quantity of fæces passed, and not due to any increase of the fluid constituent thereof.

We now come to the dog in which the large intestine was partially removed, Table XI, and in which, as we have already seen, there was a dilatation of the rectum. In spite of this latter condition the absorption of this dog differed somewhat from the absorption of the normal dog. In this dog, during the three periods analysed, the quantity of fæces fluctuated on different days, and during the first two periods

there was occasionally an interval of a day without any fæces being passed. In consequence of this constipation one would expect the percentage of water to be small; it is seen that in this dog the percentage of water during the first period was 50·96, and with a slight increase of fat rose, instead of fell, to 54·59; still further rising on the diet rich in fat to 72·46. The quantity of water also rose; but in this dog, by increase of fat in the diet, one got not only an increase in the total quantity of water eliminated by the fæces but also an increase in the percentage.

Table XI.—Partial Removal of Large Intestine, showing the Influence of an Increasing Quantity of Fat in the Diet on the Quantity of Fæces and the Water of the Fæces.

Day.	Diet.		Fæces.		
	N.	Fat.	Quantity.	Water.	
				Total.	Per cent.
	grams.	grams.	grams.	grams.	
12	6·05	11·73	70·76	47·24	66·77
13	6·05	11·73	7·50	4·88	55·02
14	6·05	11·73		no fæces	
15	6·05	11·73	63·77	42·93	67·32
16	6·05	11·73	57·72	32·15	55·69
Average	6·05	11·73	39·95	25·44	50·96
19	6·05	36·73	49·80	35·33	70·94
20	6·05	36·73		no fæces	
21	6·05	36·73	51·97	37·29	71·75
22	6·05	36·73	43·78	33·13	75·67
Average	6·05	36·73	36·39	26·44	54·59
33	6·05	51·73	54·38	39·10	64·63
34	6·05	51·73	24·60	18·97	77·12
35	6·05	51·73	39·70	27·95	70·39
36	6·05	51·73	19·69	15·11	74·20
37	6·05	51·73	53·18	40·39	75·94
Average	6·05	51·73	38·31	28·30	72·46

We now come to the dogs in which the total removal of the large intestine is carried out. We have in Table XII two groups of results, which represent two periods during which the dog received 9·71 grams of fat, and two periods in which 29·71 grams of fat were given. As in the normal dogs, we have here also an increase in the quantity of fæces, produced by increasing the quantity of fat, and also an increase in the

Table XII.—Dog 4. Total Removal of the Large Intestine, showing the Influence of an Increasing Quantity of Fat in the Diet on the Quantity of Fæces and the Water in the Fæces.

Day.	Diet.		Fæces.		
	N.	Fat.	Quantity.	Water.	
				Total.	Per cent.
	grams.	grams.	grams.	grams.	
8	6·80	9·71	56·70	40·93	72·20
9	6·80	9·71	97·82	74·68	77·36
10	6·80	9·71	76·27	60·30	79·06
11	6·80	9·71	71·42	55·51	77·73
Average	6·80	9·71	75·55	58·11	76·59
15	6·80	9·71	79·60	60·10	76·45
16	6·80	9·71	69·43	51·95	74·83
17	6·80	9·71	81·12	63·65	78·47
18	6·80	9·71	68·67	51·76	75·37
Average	6·80	9·71	74·71	56·87	76·26
22	6·80	29·71	82·56	64·55	78·19
23	6·80	29·71	84·64	66·83	78·96
24	6·80	29·71	83·09	66·91	80·53
25	6·80	29·71	84·82	67·96	80·12
Average	6·80	29·71	83·78	66·56	79·45
29	6·80	29·71	82·90	63·56	76·67
30	6·80	29·71	81·53	63·94	78·43
31	6·80	29·71	85·10	70·13	82·41
32	6·80	29·71	84·45	66·71	78·99
Average	6·80	29·71	83·49	66·09	79·13

quantity of water daily eliminated. At the same time there is an increase in the percentage of water from 76·59 and 76·26 to 79·45 and 79·13 per cent.

In dog 5, Table XIII, in which the large intestine was removed, we also have an increase in the percentage of water by increasing the fat during the two days on which it was analysed; at the same time the total quantity of water, as well as the quantity of fæces, was not increased, but this may have been due in this case, as already explained, to the fact that a perfect determination of the periods during which the fæces were collected could not be obtained.

When we compare the averages, Table XIV, of these various experiments, we find that whereas a normal dog excretes about 18 to 22 grams of fæces, containing from 13 to 14 c.c. of water per diem on the

Table XIII.—Dog 5. Total Removal of the Large Intestine, showing the Influence of an Increasing Quantity of Fat in the Diet on the Quantity of the Fæces and the Water of the Fæces.

Day.	Diet.		Fæces.		
	N.	Fat.	Quantity.	Water. Total.	Water. Per cent.
	grams.	grams.	grams.	grams.	
10	6·262	11·548	46·93	34·70	73·94
11	6·26	11·55	93·30	44·34	74·77
12	6·26	11·55	83·80	67·71	80·80
13	6·26	11·55	94·20	78·09	82·94
14	6·26	11·55	86·55	68·01	82·98
Average	6·26	11·55	74·16	58·57	79·09
23	6·286	41·55	64·55	52·06	80·65
24	6·26	41·55	54·20	46·93	86·58
Average	6·26	41·55	59·38	49·49	83·62

various amounts of fat diet, a dog, after the total removal of the large intestine, excretes no less than from 75 to 84 grams of fæces, containing from 57 to 67 c.c. of water; and further, that whereas the percentage of water in the fæces in a normal dog varies roughly between 60 and 70, in dogs without a large intestine it varies between 77 and 84 per cent.

In the case of partial removal of the large intestine, during the time in which there was constipation and a day was consequently missed in the action of the bowels, the percentage of water fell to 50·96; but on the other occasions, when the bowels were acting regularly, the percentage tended to be high, 72·46; in fact, comparing fairly with those dogs in which the large intestine was removed.

The interesting fact brought out by this experiment is that in the normal dog the increase of fat, causing an increased quantity of fæces, is accompanied by an increased elimination of water, and at the same time by a decrease in the percentage of water eliminated, and that this relative decrease is due to an increase of solids in the fæces.

Fr. Müller* had already shown that increasing the fat in the diet of a dog fed on meat caused a decrease in the percentage of water contained in the fæces. Thus a dog on 1500 grams of meat, with 30 grams of fat, passed fæces containing 69·6 per cent. of water; but on increasing the fat of the diet to 60 grams it fell to 64·9 per cent., and still further with 250 grams to only 53·0 per cent.

* Fr. Müller, *loc. cit.*, p. 360.

Table XIV.—The Influence of an Increasing Quantity of Fat in the Diet on the Quantity of Fæces and Water in the Fæces.

No.	Duration of observation.	Diet.		Fæces.		
		N.	Fat.	Quantity.	Water. Total.	Water. Per cent.
	days.	grams.	grams.	grams.	grams.	
		Average of Two Normal Dogs.				
1 (*a*)	4	4·82	12·04	18·61	12·79	70·78
(*b*)	4	4·82	32·04	20·42	13·79	67·95
(*c*)	4	4·82	62·04	22·70	14·32	67·87
2 (*a*)	4	8·00	15·20	31·67	19·90	63·26
(*b*)	4	8·00	15·20	33·62	22·03	64·60
(*c*)	4	8·00	65·19	36·26	23·67	60·52
		Average of Partial Removal of Large Intestine.				
3 (*a*)	5	6·05	11·73	39·95	25·41	50·96*
(*b*)	4	6·05	36·78	36·39	26·44	54·59*
(*c*)	5	6·05	51·73	38·31	28·30	72·46
		Average of Total Removal of Large Intestine.				
4 (*a*)	4	6·80	9·71	75·55	58·11	76·59
(*b*)	4	6·80	9·71	74·71	56·87	76·26
(*c*)	4	6·80	29·71	83·78	66·56	79·45
(*d*)	4	6·80	29·71	83·49	66·09	79·13
5 (*a*)	5	6·26	11·55	74·16	58·57	79·09
(*b*)	2	6·26	41·55	59·38	49·49	83·62

On the other hand I find that in dogs without the large intestine the increase of fat, although it is accompanied by an increase in the quantity of fæces and increase in the quantity of water, is not accompanied by any decrease of percentage of water eliminated in the fæces.

The breaking up of Fat in the Alimentary Canal in Normal Dogs, and after Partial or Complete Removal of the Large Intestine.

With regard to the breaking up of fat in the alimentary canal in the absence of the large intestine, analyses were made on each of the preceding dogs of the fat, or rather total ether extract, as regards the quantity of neutral fat, free fatty acids, fat acids as soaps, and cholesterin it contained.

* One day passed no fæces.

Table XV.—Composition of Ether Extract of Fæces. Normal Dog 2 during the first period of four days received daily 15·20 grams of Fat, while during the second period of three days daily 65·19 grams of Fat.

Day.	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soap.		Cholesterin.	
	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
14	0·831	100	0·157	18·89	0·613	73·77	0·044	5·29	0·017	2·04
15	0·539	100	0·419	77·73	0·005	0·93	0·017	3·15	0·097	18·00
16	1·139	100	0·423	37·14	0·569	49·96	0·044	3·86	0·102	8·96
17	1·084	100	0·377	34·77	0·595	54·89	0·066	6·09	0·046	4·24
Average	0·898	100	0·344	42·13	0·446	44·89	0·043	4·60	0·066	8·31
27	2·728	100	0·310	11·36	1·357	49·74	1·025	37·57	0·036	1·32
28	1·933	100	0·266	13·76	1·291	66·79	0·293	15·16	0·080	4·14
29	2·313	100	0·365	15·78	1·430	61·82	0·454	19·63	0·068	2·94
Average	2·325	100	0·314	13·63	1·359	59·45	0·591	24·12	0·061	2·80

On this point, in normal dog No. 2, the analysis was carried out during two periods, as shown in Table XV. In the first period the dog received 15·20 grams of fat daily, and four days were separately analysed; while during the second period the dog received 65·19 grams of fat, and three days were separately analysed.

We see that the quantity of free fatty acids, as compared to neutral fat, is somewhat greater on the diet poor in fat, while on the diet rich in fat it is very markedly so, being no less than 0·314 gram of neutral fat and 1·359 grams of free fatty acids. The soap is also very markedly increased, namely, from 0·043 to 0·591 gram by increasing the quantity in the fat diet; the cholesterin, however, remains the same, being 0·066 and 0·061 gram. The percentage, taking the total ether extract as 100, on the diet poor in fat, is 40·13 per cent. of fat as neutral fat, 44·89 per cent. as free fat acids, and 4·60 per cent. as fat acids in form of soaps; while, when the diet is rich in fat, and the fæces, in spite of the increased absorption, contained more than double the quantity of ether extract, these contained only 13·63 per cent. as neutral fat, 59·45 per cent. as free fat acids, and 24·12 per cent. as soaps. The percentage of cholesterin naturally differs owing to the increased quantity of total ether extract.

Table XVI. In dog 1, on a diet rich in fat, containing no less than 62 grams of fat, the total result is very much the same as that obtained in the previous table, the percentage of fat acids being in excess of neutral fat; the cholesterin, however, in this case, was somewhat higher than in the former dog.

Table XVII. When the partial removal of the large intestine was carried out during three days' analysis on a diet containing 51·73 grams of fat daily, the same holds good, the quantity of cholesterin, however, corresponding with Table XVI being no less than 0·145 gram.

Table XVIII. In the case of dog 4, in which the large intestine has been entirely removed, on a diet comparatively poor in fat—29·7 grams —the quantity of free fatty acids is very much in excess of the neutral fat, the soaps being also slightly in excess of the fat acids; the cholesterin, on the other hand, is very markedly decreased, the average being only 0·025 gram per diem.

Table XIX. Again, in dog 5, on a diet containing 41·6 grams of fat, the animal had slight diarrhœa; throughout the three days experimented on the quantity of fat acids was again in excess of the neutral fats, and the fat acids as soaps were not increased. The cholesterin, as in the former dog, was on the whole diminished, being only 0·069 gram.

In Table XX the average results of the two normal dogs are compared with that of partial and complete removal of the large intestine, and it is seen that as far as the fat is concerned the composition of the ether extract in the fæces remains practically the same, whether the large intestine is present or absent, the fat acids being in both greatly in excess

Table XVI.—Composition of Ether Extract of Fæces. Normal Dog 1, on a Diet rich in Fat, containing 62·04 grams of Fat daily.

Day.	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.		Cholesterin.	
	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
22	0·876	100	0·032	9·36	0·549	62·67	0·113	12·90	0·131	14·95
23	1·583	100	0·180	11·37	1·115	70·43	0·126	7·96		
24	0·951	100	..	..	..	..	0·180	18·93		
25	1·644	100	0·154	9·37	1·041	62·32	0·281	17·09	0·168	10·22
Average	1·264	100	0·139	10·03	0·902	65·47	0·233	14·22	0·154	11·80

Table XVII.—Composition of Ether Extract of Fæces. Partial Removal of Large Intestine on a Diet of 51·73 grams of Fat.

Day.	Ether extract.		Neutral fat.		Free fat acids.		Fat acid as soap.		Cholesterin.	
	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
36	0·762	100	0·033	4·33	0·413	58·14	0·086	11·29	0·199	26·12
37	—	—	—	—	—	—	—	—	—	—
38	0·553	100	0·088	15·91	0·316	57·14	0·071	12·84	0·110	19·89
39	1·602	100	0·313	19·54	0·784	48·94	0·378	23·59	0·126	7·87
Average of three days	0·972	100	0·145	13·16	0·514	54·74	0·178	15·91	0·145	17·96

Table XVIII.—Composition of Ether Extract of Fæces. Complete Removal of the Large Intestine, the Diet containing 29·7 grams of Fat daily.

Day.	Ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.		Cholesterin.	
	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
29	1·248	100	0·169	13·54	0·628	50·32	0·452	36·22		
30	0·660	100	0·082	12·43	0·427	64·70	0·123	18·64	0·027	4·09
31	0·710	100	0·158	22·25	0·452	63·66	0·074	10·42	0·026	3·66
32	0·585	100	0·085	14·53	0·408	69·74	0·070	11·97	0·022	3·76
Average	0·801	100	0·124	15·69	0·479	63·11	0·180	19·31	0·025	3·84

Table XIX.—Composition of the Ether Extract of Fæces. Complete Removal of the Large Intestine, the Diet containing 41·6 grams of Fat.

Day.	Total fat.		Neutral fat.		Free fat acids.		Fat acids as soaps.		Cholesterin.	
	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
23	0·936	100	0·144	15·38	0·443	47·33	0·281	30·02	0·068	7·27
24	0·701	100	0·136	19·40	0·464	66·19	0·024	3·42	0·076	10·84
25	0·507	100	0·070	13·81	0·333	65·68	0·042	8·28	0·062	12·23
Average	0·715	100	0·116	16·20	0·413	59·73	0·166	13·91	0·069	10·11

Table XX.—The Effect of Removal of the Large Intestine on the breaking up of Fat in the Alimentary Canal. Composition of Fat of Fæces.

No.	Duration of observation.	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.		Cholesterin.	
		Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
	days.										
Average of Two Normal Dogs.											
1	4	1·264	100	0·139	10·03	0·902	65·47	0·233	14·22	0·154	11·80
2	3	2·325	100	0·314	13·63	1·359	59·45	0·591	24·12	0·061	2·80
Average of Partial Removal of Large Intestine.											
3	3	0·972	100	0·145	13·16	0·514	54·74	0·178	15·91	0·145	17·96
Average of Two Dogs after Complete Removal of the Large Intestine.											
4	4	0·801	100	0·124	15·69	0·479	62·11	0·180	19·31	0·025	3·84
5	3	0·715	100	0·116	16·20	0·413	59·73	0·116	13·91	0·069	10·11

of the neutral fat and the fat acids as soaps. In some the quantity of fat acids combined as soaps is much greater than the neutral fat, and in others they are very much alike; but at any rate the large intestine appears to have no influence whatever in altering the composition of the fats in the fæces. On the other hand there is a difference in the quantity of cholesterin present.

It is seen that in the second normal dog the quantity of cholesterin differs considerably from that seen in the first, and that results seen in the partial removal of the large intestine correspond with the normal dogs. On the other hand, in the two dogs in which the large intestine was completely removed, the cholesterin found was small in quantity; and if the averages are taken it is seen that the removal of the large intestine tends to cause a decrease in the quantity of cholesterin daily eliminated in the fæces.

It has already been shown by Jankau* that large quantities of cholesterin given by the mouth are absorbed, and therefore it is not at all surprising if the quantities of cholesterin which are daily eliminated in the bile, &c., are to a certain extent absorbed in the small intestine, that the absence of the large intestine should have no effect in increasing the quantity found in the fæces. At the same time, the fact that the actual quantity of cholesterin found in the fæces appears to be smaller in dogs without a large intestine, would appear to be explainable by the fact that the secretion of the large intestine itself contains cholesterin. Further, it is conceivable that it is not so readily absorbed by the large intestine, hence the quantity of cholesterin normally present is larger than that found when the large intestine is absent.

Before leaving the subject of the fæces, it is of interest to note their alteration as regards colouring matter after the removal of the large intestine.

The Action of the Removal of the Large Intestine on the Urobilin Formation in the Fæces.

In a paper already published I showed† how it seemed probable that the presence of urobilin in the urine was due to the conversion of the bile pigments during their passage along the intestines.

Schmidt‡ showed that when a concentrated solution of perchloride of mercury is applied to wet or dry fæces containing urobilin, in the space of a few minutes a bright rose-red colour is developed. The rose-coloured extract, when separated and examined with the spectroscope, shows the urobilin band between F and *b*. This test is all the

* Jankau, 'Archiv f. Exp. Path. u. Pharm.,' vol. 29, p. 237, 1892.

† Vaughan Harley, 'Brit. Med. Journ.,' Oct. 3rd, 1896.

‡ Schmidt, 'Verhandlungen d. Congress f. Universale Medicin,' p. 320, 1895.

more valuable since in the presence of bile it gives a bright green colour, in consequence of the conversion of bilirubin into biliverdin by the perchloride of mercury.

Since this method has been introduced it has become possible to recognise very small quantities of urobilin in the fæces.

Schmidt, examining by this method the different parts of the intestine and intestinal walls, showed that the urobilin reaction yielded results proving that in different parts of the intestine urobilin was present in varying degree, and also varied in different cases. The staining of the intestinal walls by this method, he considered, indicated the parts of the intestine which took part in the absorption of urobilin. My own experiments on dogs and monkeys, in which the post mortem examination was made immediately after death, showed no such staining of the walls, and therefore I consider it was probably due to a post mortem diffusion.

The test, however, is valuable for the determination of urobilin in the contents of the bowel, and this we will now consider.

I noted in various animals that as a rule no trace of urobilin could be found in the contents of the intestine above the ileo-cæcal valve. In some few dogs there was a trace of urobilin in the contents just above the cæcum, but in the majority of normal dogs recognition of this substance was only possible after the ileo-cæcal valve was passed.

Of the dogs in which the large intestine had been removed, dog 5 was killed by chloroform and the contents examined. In this dog the post mortem showed the small intestine to have been artificially joined to the rectum 6 cm. from the anus; and 78 cm. from the anus the green colour was first noticed by the perchloride of mercury reaction, and then, 35 cm. from the anus, began to show the urobilin reaction, which extended as far as the anus.

In dog 4 only 4 cm. of the rectum was found remaining at the post mortem, and this dog only showed a very faint urobilin reaction in the contents of this latter 4 cm. That is to say, throughout the entire small intestine of this dog no reaction of urobilin could be obtained.

These post mortem results were borne out to a certain extent by observations during life. It was found in most of the dogs, in which the large intestine had been removed, that immediately after the operation the motions were bile-stained, and gave the green colour with perchloride of mercury, and, only very faintly or not at all, the pink urobilin reaction. Later on the urobilin became increased, and the bilirubin apparently decreased.

In dog 5, throughout the time metabolism experiments were carried on, there was always more marked urobilin than in other dogs observed after the removal of the large intestine.

The Influence of Diet on the Total Alkaline and Aromatic Sulphates in Normal Dogs and in those in which the Large Intestine has been in part or completely Removed.

Having discussed the fæces, we now come to consider the sulphates in the urine. As we all know, the sulphur taken in the diet is principally excreted in the form of sulphates, either combined with alkalis or with aromatic substances, although some 14 to 25 per cent. of the total sulphur in the urine is excreted as neutral sulphides.*

In this paper the sulphides were not determined. The sulphates contained in the urine are principally derived from the proteids in the diet, as only very small quantities are taken in the form of salts, and, in consequence, the ratio of sulphuric acid to nitrogen excreted in the urine remains very parallel—5 : 1. Hence it will be seen in the following tables that the proteid-sparing action of the increase of fat causes not only a decrease in the nitrogen in the urine in normal dogs but also a decrease in the quantity of the sulphates. The sulphur of the proteids in fact had been retained in the organism for building up the proteid of the bòdy itself, in the same way as the nitrogen had been.

The aromatic sulphates were also investigated after removal of the large intestine in order to see if the intestinal putrefaction was in any way influenced by the removal of the large intestine; for the experiments of Salkowski,† Baumann,‡ &c., have shown that in all probability the phenol, indol, &c., which, when absorbed into the blood, go to form the aromatic sulphates, are only formed in the large intestine.

Baumann and Ewald§ have described cases of intestinal fistula; during the time in which the fæces were excreted through the fistula the quantity of aromatic sulphates was very much diminished in the urine.

Table XXI. In normal dogs the analyses of the sulphates were carried out during three periods in which the dog received progressively an increase of fat in his diet.

We have already seen that this increase of fat caused a decrease in the quantity of urine and in the quantity of nitrogen daily eliminated. Accompanying this, we find in the above table there is also a decrease in the quantity of the alkaline sulphates progressively with the increase of fat. On the other hand, the aromatic sulphates are not influenced by the increase of fat to 32·04 grams per diem; but on increasing the fat to 62·04 grams there is an apparent decrease in the aromatic sulphates.

* v. Noorden, 'Lehrb. d. Path. Stoff.,' p. 283.

† Salkowski, 'Zeit. f. Phys. Chemie,' vol. 10, p. 266, 1886.

‡ Baumann, *ibid.*, vol. 10, p. 126, 1886.

§ Ewald, 'Arch. f. Path. Anat.,' vol. 75, p. 409.

Table XXI.—Normal Dog 1. The Influence of an Increasing Quantity of Fat in a Fixed Proteid and Carbohydrate Diet on the Sulphates in the Urine.

Day.	Weight.	Diet.		Urine.			Sulphates.		
		N.	Fat.	Quantity.	N.	Total.	Alkaline, A.	Aromatic, B.	A : B.
	kilos.	grams.	grams.	c.c.	grams.	grams.	grams.	grams.	
8	4·59	4·82	12·04	125	4·382	0·663	0·617	0·046	11 : 1
9	4·59	4·82	12·04	130	4·810	0·619	0·558	0·061	9 : 1
10	4·59	4·82	12·04	120	4·399	0·625	0·557	0·068	8 : 1
11	4·59	4·82	12·04	95	4·236	0·642	0·561	0·081	7 : 1
Average...	4 59	4·82	12·04	118	4·457	0·637	0·573	0·064	9 : 1
15	4·59	4 82	32·04	95	3·754	0·564	0·489	0·075	7 : 1
16	4·59	4·82	32·04	94	3·625	0·591	0·504	0·087	7 : 1
17	4·59	4·82	32·04	80	3·164	0·424	0·380	0·044	9 : 1
18	4·59	4·82	32·04	85	3·575	0·544	0·543	0·055	9 : 1
Average...	4·59	4·82	32·04	89	3·575	0·544	0·479	0·065	8 : 1
22	4·63	4·82	62·04	70	3·235	0·483	0·428	0·054	8 : 1
23	4·63	4·82	62·04	65	3·340	0·483	0·447	0·058	8 : 1
24	4·63	4·82	62·04	60	3 085	0·526	0·472	0·054	9 : 1
25	4·63	4·82	62·04	85	3·789	0·569	0·512	0·058	9 : 1
Average...	4·63	4·82	62·04	70	3·362	0·521	0·465	0·056	8·5 : 1

The decrease in the alkaline sulphates and no alteration in the quantity of aromatic sulphates during the second period causes the ratio A to B to fall from 9 : 1 to 8 : 1, that is to say, by discussing only these ratios, one would consider that the intestinal putrefaction had really been increased instead of decreased. However, on increasing the fat in period 3, the decrease in the aromatic sulphates was sufficient to counteract the decrease in total sulphates, so that the ratio was 8·5 : 1.

On turning to Table XXII, normal dog 2, we see the increase of fat in the food causes, as in the preceding dog, a decrease in the quantity of urine, and at the same time a decrease in the quantity of nitrogen excreted; there is also in this case a more marked decrease in the quantity of alkaline sulphates, but this is due to the greater decrease in the quantity of nitrogen. The aromatic sulphates, however, are slightly decreased, the normal ratios being 7 : 1, while the ratio on the increased fat diet is 6·6 : 1. We here have also, even in spite of a decrease of aromatic sulphates produced by increasing the fat in the diet (since the alkaline sulphates are more markedly decreased), a smaller ratio, which would lead us to believe that intestinal putrefaction is increased.

From these two dogs we see that the old idea that the ratio 8 or 10 : 1 should be considered as the normal ratio indicating intestinal putrefaction must be corrected, and that in future it is not sufficient to make out the ratio of the total day's urine, but it is essential to compare the quantities in a given space of time, for on a diet rich in fat the ratio may be very much diminished, say to 6 : 1, in spite of the aromatic sulphates being also diminished.

It would further appear that in a normal dog fat added to the diet, if anything, tends to decrease the amount of aromatic sulphates, so that the increase of fat does not cause an increase of intestinal putrefaction.

Table XXIII. We now come to discuss the dog in which the large intestine was in part previously removed. In this case we have the same decrease in water and nitrogen in the urine by an increase in the fat as in normal dogs, and also a decrease in the alkaline sulphates and aromatic sulphates. In fact, the results, as far as the sulphates are concerned, obtained in this dog, correspond exactly with those found in the two normal dogs, so that we can conclude the partial removal of the large intestine has no effect on the sulphates.

Table XXIV. In dog 4 the effect of the total removal of the large intestine was investigated twice on two diets. In the first the diet contained 9·71 grams of fat, and in the second 29·71 grams.

We see in this dog that increasing the fat causes, as in the normal dog, a decrease in water and nitrogen in the urine, and this is also accompanied by a decrease in the alkaline sulphates, but no alteration

Table XXII.—Normal Dog 2. The Influence of an Increasing Quantity of Fat in a Fixed Proteid and Carbohydrate Diet on the Sulphates of the Urine.

Day.	Weight.	Diet.		Urine.			Sulphates.		
		N.	Fat.	Quantity.	N.	Total.	Alkaline.	Aromatic.	A : B.
	kilos.	grams.	grams.	c.c.	grams.	grams.	grams.	grams.	
9	6·41	8·00	15·20	130	6·950	0·681			
10	6·41	8·00	15·20	130	..	0·772	0·678	0·094	7 : 1
11	6·41	8·00	15·20	100	5·972	0·625	0·560	0·065	8 : 1
12	6·41	8·00	15·20	115	5·460	0·569	0·500	0·069	7 : 1
Average ..	6·41	8·00	15·20	119	6·127	0·662	0·579	0·076	7 : 1
14	6·41	8·00	15·20	90	5·530	0·639	0·558	0·081	6 : 1
15	6·41	8·00	15·20	85	5·159	0·567	0·491	0·076	7 : 1
16	6·41	8·00	12·20	115	6·314	0·714	0·620	0·094	6 : 1
17	6·40	8·00	15·20	143	6·258	0·797	0·720	0·077	9 : 1
Average ..	6·41	8·00	15·20	108	5·815	0·679	0·597	0·082	7 : 1
21	6·47	8·00	65·19	93	4·190	0·502	0·437	0·066	7 : 1
22	6·52	8·00	65·19	50	3·115	0·335	0·286	0·049	6 : 1
23	6·63	8·00	65·19	80	4·270	0·498	0·413	0·085	5 : 1
Average...	6·59	8·00	65·19	74	3·858	0·445	0·377	0·066	6 : 1

Table XXIII.—Partial Removal of the Large Intestine. The Influence of an Increasing Quantity of Fat to a Fixed Proteid and Carbohydrate Diet on the Sulphates of the Urine.

Day.	Weight.	Diet.		Urine.			Sulphates.		
		N.	Fat.	Quantity.	N.	Total.	Alkaline, A.	Aromatic, B.	A : B.
	kilos.	grams.	grams.	c.c.	grams.	grams.	grams.	grams.	
12	6·18	6·05	11·73	210	5·930	0·861	0·716	0·085	8 : 1
13	6·18	6·05	11·73	200	5·601	0·749	0·685	0·064	10 : 1
14	6·18	6·05	11·73	180	5·873	0·792	0·719	0·073	9 : 1
15	6·18	6·05	11·73	135	5·333	0·715	0·641	0·074	9 : 1
16	6·18	6·05	11·73	135	5·244	0·734	0·663	0·061	10 : 1
Average...	6·18	6·05	11·73	172	5·596	0·768	0·685	0·072	9 : 1
19	6·19	6·05	36·73	160	5·491	0·796	0·713	0·083	8 : 1
20	6·20	6·05	36·73	216	5·251	0·853	0·766	0·087	8 : 1
21	6·26	6·05	36·73	150	4·188	0·561	0·499	0·061	8 : 1
22	6·26	6·05	36·73	150	5·032	0·697	0·618	0·079	8 : 1
Average...	6·23	6·05	36·72	169	4·991	0·727	0·649	0·077	8 : 1
30	6·68	6·05	51·73	120	4·307	0·710	0·632	0·077	8 : 1
31	6·75	6·05	51·73	112	4·763	0·794	0·738	0·056	13 : 1
32	6·75	6·05	51·73	130	4·971	0·612	0·540	0·072	7 : 1
33	6·75	6·05	51·73	100	..	0·609	0·549	0·060	9 : 1
Average...	6·74	6·05	51·73	112	(4·680)	0·681	0·615	0·069	9 : 1

Table XXIV.—Total Removal of Large Intestine. Dog. 4. The Influence of an Increasing Quantity of Fat in a Fixed Proteid and Carbohydrate Diet on the Sulphates in the Urine.

Day.	Weight.	Diet.		Urine.			Sulphates.		
		N.	Fat.	Quantity.	N.	Total.	Alkaline, A.	Aromatic, B.	A : B.
	kilos.	grams.	grams.	c.c.	grams.	grams.	grams.	grams.	
8	4·05	6·80	9·71	110	4·437	0·620	0·588	0·032	18 : 1
9	4·05	6·80	9·71	170	4·812	0 675	0·640	0·033	19 : 1
10	4·05	6·80	9·71	210	4·120	0·555	0·518	0·037	14 : 1
11	4·05	6·80	9·71	275	4·418	0·543			
Average	4·05	6·80	9·71	191	4·445	0·598	(0·582)	(0·034)	17 : 1
15	4·05	6·80	9·71	397	4·405	0·541	0·516	0·024	22 : 1
16	4·05	6·80	9·71	309	4·290	0·564	0·534	0·034	17 : 1
17	4·11	6·80	9·71	320	4·427	0·645	0·624	0·023	27 : 1
Average	4·08	6·80	9·71	341	4·374	0·584	0·558	0·027	22 : 1
22	4·17	6·80	29·71	121	3·010	0·392	0·568	0·024	15 : 1
23	4·17	6·80	29·71	200	3·282	0·469	0·439	0·030	14 : 1
24	4·17	6·80	29·71	240	3·344	0·437	0·401	0·036	11 : 1
25	4·17	6·80	29·71	265	3·337	0·447	0·411	0·035	12 : 1
Average	4·17	6·80	29·71	207	3·243	0·437	0·405	0·031	13 : 1
29	4·24	6·80	29·71	52	1·230	0·220	0·194	0·026	7 : 1
30	4·30	6·80	29·71	210	3·220	0·448	0·411	0·036	11 : 1
31	4·31	6·80	29·71	190	3·690	0·462	0·427	0·035	12 : 1
32	4·31	6·80	29·71	140	3·620	0·493	0·458	0·034	13 : 1
Average	4·29	6·80	29·71	198	2·965	0·406	0·372	0·033	11 : 1
Average of three last days }	..	..	..	247	3·510	0·468	0·432	0·035	12 : 1

in the amount of aromatic sulphates. The total quantity of aromatic sulphates in this dog is very much decreased, being only from 0·026 to 0·035 gram as against 0·064 to 0·082 gram in the normal dogs. We therefore see that the absence of the large intestine in this case caused a very marked decrease in the amount of aromatic sulphates, and this decrease in the amount of aromatic sulphates was, doubtless, due to the decrease of intestinal putrefaction, and explains how it was that urobilin was found in such small quantities in the fæces.

As far as the ratio of alkaline to aromatic sulphates is concerned, in the two periods with the small quantity of fat the ratio is 17 : 1 and 22 : 1, while during the two periods with the large quantity of fat the ratio was 13 : 1 to 12 : 1.

We have here even more markedly than in the normal dog the error brought out by only noting the ratio, and not the absolute quantity, of aromatic sulphates, for with a ratio of 12 : 1, as compared to that of 22 : 1, we should think the aromatic sulphates were very markedly increased if we did not know that this alteration in the ratio was due, not to an increase of the aromatic sulphates and therefore increase in the intestinal putrefaction, but simply to the decrease of alkaline sulphates in consequence of the proteid-sparing action of the increased fat in the diet.

In Table XXV, dog 5, only two periods were investigated. Here we see that a very marked diminution in the quantity of nitrogen was produced by increasing the fat from 11·55 to 41·55 grams. Accompanying this is an equal diminution in the quantity of alkaline sulphates. The aromatic sulphates in this case also were decreased in amount by increasing the fat; in consequence of this, the ratio in this dog remains much the same.

In Table XXVI are placed together the averages obtained in the preceding dogs, and it is seen that they compare very well with one another.

As a general conclusion, one may say that in all cases the increase of fat causes a decrease in the quantity of sulphates, and at the same time tends to decrease the quantity of aromatic sulphates; that in the absence of the large intestine this still holds good, although the aromatic sulphates are extremely small in amount.

It is thus useless to consider the ratio of alkaline to aromatic sulphates as an indication of the amount of intestinal putrefaction, unless the diet is poor in fat.

It is also important to note that the amount of proteid itself given in the diet does not influence the amount of aromatic sulphates nearly as much as one would expect, for in dogs 1, 2, and 3, in spite of the proteid varying in quantity very considerably, the quantity of aromatic sulphates remained the same.

Table XXV.—Dog 5. Total Removal of the Large Intestine. The Influence of an Increasing Quantity of Fat in a Fixed Proteid and Carbohydrate Diet on the Sulphates in the Urine.

Day.	Weight.	Diet.		Urine.			Sulphates.		
		N.	Fat.	Quantity.	N.	Total.	Alkaline, A.	Aromatic, B.	A : B.
	kilos.	grams.	grams.	c.c.	grams.	grams.	grams.	grams.	
10	6·30	6·26	11·55	90	4·508	0·509	0·419	0·059	7 : 1
11	6·30	6·26	11·55	86	4·110	0·391	0·342	0·049	7 : 1
12	6·30	6·26	11·55	120	4·461	0·416	0·364	0 051	7 : 1
13	6·30	6·26	11·55	96	4·343	0·491			
14	6·30	6·26	11·55	75	4·459	0·422	0·374	0·048	8 : 1
Average...	6·30	6·26	11·55	93	4·376	0·414	0·382	0·052	7 : 1
23	6·63	6·26	41·55	96	2·422	0·215	0·183	0·032	6 : 1
24	6·62	6·26	41·55	85	2·408	0·234	0·203	0·031	7 : 1
25	6 53	6·26	41·55	82	2·748	0·246	0·216	0·029	7 : 1
Average...	6·59	6·26	41·55	88	2·256	0·232	0·201	0·031	6·6 : 1

Table XXVI.—The Averages of Normal Dogs as compared with Partial and Complete Removal of the Large Intestine, showing the Influence of Increasing Quantity of Fat in a Fixed Proteid and Carbohydrate Diet on the Sulphates in the Urine. Nos. 1 and 2, Normal Dogs. No. 3, Partial Removal of the Large Intestine. Nos. 4 and 5, Complete Removal of the Large Intestine.

No.	Duration of observation.	Diet.		Urine.		Sulphates.			
		N.	Fat.	Quantity.	N.	Total.	Alkaline, A.	Aromatic, B.	A : B ratio.
	days.	grams.	grams.	c.c.	grams.	grams.	grams.	grams.	
1 (*a*)	4	4·82	12·04	118	4·457	0·637	0·573	0·064	9 : 1
(*b*)	4	4·82	32·04	89	3·573	0·544	0·479	0·065	8 : 1
(*c*)	4	4·82	62·04	70	3·362	0·521	0·465	0·056	8·5 : 1
2 (*a*)	4	8·00	15·20	119	6·127	0·662	0·558	0·076	7 : 1
(*b*)	4	8·00	12·20	108	5·815	0·679	0·597	0·082	7 : 1
(*c*)	3	8·00	65·19	74	3·858	0·445	0·377	0·066	6 : 1
3 (*a*)	5	6·05	11·73	172	5·596	0·768	0·685	0·072	9 : 1
(*b*)	4	6·05	36·73	169	4·991	0·727	0·649	0·077	8 : 1
(*c*)	4	6·05	51·73	112	4·680	0·681	0·615	0·065	9 : 1
4 (*a*)	4	6·80	9·71	191	4·445	0·598	0·582	0·034	17 : 1
(*b*)	3	6·80	9·71	341	4·374	0·584	0·588	0·026	22 : 1
(*c*)	4	6·80	29·71	207	3·243	0·437	0·405	0·031	13 : 1
(*d*)	3	6·80	29·71	247	3·510	0·468	0·432	0·035	12 : 1
5 (*a*)	5	6·26	11·55	93	4·376	0·382	0·052	0·052	7 : 1
(*b*)	3	6·26	41·55	88	2·526	0·232	0·201	0·031	6·6 : 1

Summary.

The conclusions to be drawn from the above experiments can be briefly summarised, so that the general results will be more readily seen.

1. The large intestine itself excretes a substance which contains proteids, fat, and salts, thus resembling "Hermann's loop," but at the same time contains no colouring matter, and so differs from the contents of the small intestine.

2. Increasing the quantity of fat on a standard diet in normal animals leads to a decrease in the quantity of urine passed, together with a decrease in the amount of nitrogen eliminated in the urine, but tends to increase its specific gravity. The quantity of fæces is increased when the quantity of fat in the diet is increased, and this is accompanied with an increase in the nitrogen eliminated, and a more marked increase in the quantity of fat eliminated from the bowel.

In consequence of the increase in the nitrogen in the fæces, there is a decrease in the amount of nitrogen absorbed from the alimentary canal, but in spite of the increase in the quantity of fat in the fæces, the increased quantity in the diet leads to an increase in the amount absorbed. The animal's weight increases, and this is due in part to the increase of fat tissue by the additional fat in the diet, but mostly to the fresh fat acting as a sparer of proteid destruction in the organism. (Table III.)

3. That increasing the quantity of fat in a standard diet in an animal after partial removal of the large intestine yields similar results as regards the urine and general metabolism to those found in normal dogs, except that the increased quantity of fat in the diet does not appear to cause an increase in the amount of the fæces, or an increase in the nitrogen of the fæces, and only a slight increase of fat in the fæces.

The partial removal of the large intestine causes a diminution in the absorption of nitrogen from the alimentary canal, which is only slightly influenced by increasing the fat in the diet. The absorption of fat seems to be influenced in the same manner as in normal dogs by increasing the fat in the diet. (Table V.)

4. The increased quantity of fat in a standard diet in dogs, after total removal of the large intestine, causes a decrease in the quantity of urine, and nitrogen in the urine, with a tendency to an increase in specific gravity.

The quantity of fæces is increased by increasing the fat in the diet, but the quantity of nitrogen and fat in the fæces is uninfluenced by the larger amount of fat taken, so that these dogs do not correspond to what is found in normal dogs. (Table VIII.)

5. The influence of increasing quantities of fat in the diet on the quantity of water in the fæces.

In normal dogs it is found that the increase of fat in the diet causes a progressive increase in the total quantity of water eliminated in the fæces, while the percentage of water is decreased, the total quantity of the fæces increasing with the increase of fat in the diet.

In partial removal of the large intestine there is also an increase in the total quantity of water, as well as apparently an increase in the percentage of water.

In complete removal of the large intestine there is an increase in the total quantity of water eliminated in the fæces, with an increase in the percentage of water eliminated by the bowel, the fæces increasing in quantity with the augmentation in the quantity of fat in the diet.

It is thus seen that increase of fat causes an increased quantity of fæces in the normal dog, the increased quantity of fæces is accompanied by an increase of total water, but a decrease in the percentage of water eliminated in the fæces, while in the case of the removal of the large intestine, the increase in the quantity of fæces is accompanied both with an increase in the total quantity of water and the percentage of water. (Table XIV.)

6. The influence of the removal of the large intestine on the absorption from the alimentary canal.

It has been already seen that the dogs passed a larger quantity of fæces when the large intestine had been removed, and this is to a small extent brought out in partial removal of the large intestine. It is seen that the increase in quantity of fæces is principally due to the increase in quantity of water, the total quantity being nearly five times as much as in the case of the normal dogs. The quantity of nitrogen in the fæces is increased to nearly three times as much as in the normal dogs, while the quantity of fat remains unaltered, and in those cases which were examined the fæces contained no carbohydrates.

It is therefore seen that as far as the absorption from the alimentary canal is concerned :—

(*a*) The carbohydrates are absorbed equally well with and without the presence of the large intestine.

(*b*) The fats are also absorbed equally well. The normal dogs show a percentage of absorption from 94 to 98, according to the amount of fat given. This apparently better absorption occurs with the increase of fat in the diet.

In partial removal of the large intestine the percentage is roughly 96, while when the entire large intestine is removed, from 92 to 98 per cent. of the fat given is absorbed, so that the dogs with and without the large intestine appear to absorb fat equally well.

(*c*) The proteids, as indicated by the nitrogen in the fæces, are, however, very markedly influenced. In normal dogs 93 to 98 per cent.

of the nitrogen given in the diet was absorbed, the quantity diminishing with the increased quantity of fat.

In partial removal of the large intestine from 89 to 90 per cent. of the nitrogen was absorbed, the quantity not decreasing as in the normal dogs with the increase of the fat in the diet. When the entire large intestine is removed only 84 per cent. of the nitrogen given in the diet was absorbed. The quantity absorbed is uninfluenced by the increase of fat in the diet.*

One may therefore conclude that 10 per cent. at least of the nitrogen in the diet is absorbed by the large intestine, and in all probability a very much larger quantity, as we have seen the large intestine itself excretes a nitrogen-containing substance. (Tables III, V, VIII.)

7. The effects of removal of the large intestine on the breaking up of fat in the alimentary canal.

In comparing the separate analyses of the fat contained in the fæces, it is found the fat acids, neutral fat, and fat acids present as soaps remain practically the same in dogs with and without the large intestine. It would, however, appear that the quantity of cholesterin tends to decrease in the fæces in the absence of the large intestine. (Table XX.)

8. The action of the removal of the large intestine on urobilin formation.

In normal dogs the fæces were found to contain no bile, but large quantities of urobilin, while when the large intestine was removed this was not always the case, as in some, especially soon after the operation, large quantities of bile pigment would be recognised in the fæces with little or no urobilin.

On examining the walls of the intestine it was found that the urobilin reaction in normal dogs as a rule could only be obtained beyond the ileo-cæcal valve. In two dogs in which the large intestine was removed only a slight urobilin reaction was discovered in the ilium.

9. The influence of fat on the total alkaline and aromatic sulphates.

In normal dogs increasing the quantity of fat in the diet causes, with the decrease in the quantity of nitrogen in the urine, a corresponding diminution in the quantity of total sulphates. This steady decrease in the total sulphates is not due to a diminution in the quantity of the aromatic sulphates, but of the alkaline sulphates, since the aromatic sulphates are only very slightly decreased.

In consequence of this decrease in the alkaline sulphates the ratio of A : B is decreased, so that if one only referred to the ratio one would be led to believe that there was an increase in the intestinal putrefaction, while in reality there is no increase but rather a decrease, as indicated by the slight diminution in the quantity of aromatic sulphates. (Table XX. Dogs 1 and 2.)

* Experiment 5 (*b*) is admitted for the reasons already given.

10. The influence of removal of the large intestine on the total alkaline and aromatic sulphates.

The total sulphates are influenced in the same manner in the dogs without the large intestine as in normal animals by increasing the fat in the diet. The alkaline sulphates decrease with the diminution in the quantity of nitrogen eliminated in the urine. This diminution is due to the decrease in the alkaline sulphates, the same as in the normal dogs.

The increased quantity of fat in the diet has no influence apparently on the quantity of aromatic sulphates eliminated in the urine. The removal of the large intestine tends markedly to diminish the quantity of aromatic sulphates daily eliminated, the quantity eliminated being less than half the quantity found in a normal dog on the same diet; so that the removal of the large intestine evidently causes a marked diminution in the intestinal putrefaction, or rather has removed the principal seat for intestinal putrefaction. (Table XXVI. Dogs 4 and 5.)

February 2, 1899.

The LORD LISTER, F.R.C.S., D.C.L., in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Sets of Operations in Relation to Groups of Finite Order." By A. N. WHITEHEAD, M.A. Communicated by Professor A. R. FORSYTH, F.R.S.

II. "Note on the Enhanced Lines in the Spectrum of α Cygni." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

III. "On the Effects of Strain on the Thermo-electric Qualities of Metals." By MAGNUS MACLEAN, M.A., D.Sc. Communicated by LORD KELVIN, F.R.S.

IV. "The Constitution of the Electric Spark." By ARTHUR SCHUSTER, F.R.S., and G. HEMSALECH.

"On the Refractive Indices and Densities of Normal and Semi-normal Aqueous Solutions of Hydrogen Chloride and the Chlorides of the Alkalis." By Sir JOHN CONROY, Bart., M.A., F.R.S., Fellow and Bedford Lecturer of Balliol College, and Millard Lecturer of Trinity College, Oxford. Received December 15, 1898,—Read January 19, 1899.

A very large number of observations have been made of the refractive indices and densities of aqueous solutions of inorganic salts and acids: in England, more especially, by Dr. J. H. Gladstone, who in a paper in the 'Philosophical Transactions' for 1870, gave the values he had obtained for the refractive indices and densities of some 160 salts and acids; and in a series of papers published subsequently in the 'Journal of the Chemical Society,' has given the results of further observations.

Most, however, of these determinations have been made with solutions of different strengths, and at different temperatures, and, therefore, I venture to bring before the Royal Society an account of some observations I have made of the refractive indices and densities of normal and semi-normal aqueous solutions of hydrogen chloride, and the chlorides of the alkalis at a uniform temperature of 18°.

The method was the ordinary hollow prism one, but special care was taken to keep the solution at a definite known temperature. An alteration of 1° in the temperature of water in the neighbourhood of 20° makes a difference in the refractive index of nearly one unit in the fourth decimal place.*

The goniometer used was made by Messrs. Troughton and Simms; it has an 8-inch circle divided into 10′, and is read by means of two micrometers, directly to 10″, and by estimation to single seconds. The prism was made by Steinheil; the value of its refracting angle, as determined by ten independent measurements, was 60° 1′ 44″. The prism table was supported independently of the divided circle by means of a steel axis.

In order that the temperature of the prism might be kept constant, it was surrounded by a water-jacket, the prism being in actual contact with the metal casing containing the water, as, in some previous experiments,† it had been found that when the prism was merely surrounded by the water-jacket, without being in actual contact with it, a considerable time elapsed before the prism and water-jacket were

* *Conf.* Landolt and Börnstein's 'Tables,' p. 419.

† "On the Refractive Index of Water at Temperatures between 0° and 10°," 'Roy. Soc. Proc.,' vol. 58, p. 228.

at the same temperature. In order that the prism might be in contact with the water-jacket it was necessary that this should be carried by the table of the goniometer, and therefore that it should be as small and light as possible.

A slow stream of water from a tap was allowed to flow through a coil of about 5 metres of "compo" pipe, placed in a metal water-bath supported over a small gas flame; the temperature of the water being kept constant by means of a Harcourt gas regulator. From the coil the water passed through a rubber tube to the water-jacket. This consisted of two flat brass boxes 1·5 cm. deep and 12 cm. in diameter, between which the prism was placed; the lower box being circular, the upper annular. The boxes were fixed together concentrically and their interiors connected by two vertical brass tubes 5·2 cm. long. The brass plate which formed the top of the lower box was 5 mm. thick and 15 cm. in diameter; the levelling screws, by which the water-jacket was supported on the table of the goniometer, worked in holes drilled in the overhanging edge of the plate, and the prism rested on it.

A brass ring with a screw cut on its inner surface was fixed round the top of the central opening in the upper box, a brass tube 2·5 cm. in internal diameter, with a screw cut on its outer surface, worked into the thread on the ring, and by screwing this tube down on to the top of the prism, the prism could be held in position. The object of this particular arrangement was to enable the prism to be filled and emptied by means of a pipette, without interfering with its position, and to allow a thermometer, and a platinum stirring wire, to be introduced into the liquid whose refractive index was to be observed.

The third face of the prism was in actual contact with the flat surface of one of the vertical tubes, by means of which the two boxes were connected together. The cross section of these two tubes was such, that the prism was, as far as possible, surrounded by the water-jacket. Paper tubes, fitted over the ends of the collimator and telescope, projected into the openings in the water-jacket. By packing the spaces between these paper tubes and the water-jacket with cotton wool, the sides of the prism were, to a considerable extent, shielded from air currents, without preventing the adjustment of the prism and telescopes.

Two small metal pipes were fixed to the top of the upper cylindrical box, the one ending just within the box, the other reaching down through one of the connecting tubes already mentioned, nearly to the bottom of the lower box. To these metal pipes rubber tubes were attached; through the one the water from the coil flowed into the jacket, through the other from the jacket to the waste.

By keeping the temperature of the water-bath at about 20° (the temperature necessary varied with the temperature of the room, an

underground one), the temperature of the liquid in the prism could be easily kept within one or two tenths of a degree of 18°.

The liquid in the prism was kept stirred by means of the platinum wire, the end of which was bent round into a ring, and the bulb of the thermometer was immersed in the liquid.

The observations were made with sodium light only, as it had been previously found that, owing to the brilliancy and constancy of the sodium light, it was not only far easier to make observations with it, but that those observations would be more accurate than observations made with light of other refrangibilities.

The readings were made by moving the telescope first from left to right, then from right to left, until the intersection of the cross wires coincided with the image of the sodium line; and the micrometer was read in the same way. After four readings of the micrometer had been made, the prism was reversed by rotating the table of the goniometer, and adjusted for minimum deviation: four readings made, and then the prism again adjusted for minimum deviation, and four more readings made; the prism replaced in its original position, adjusted, and the same number of readings made.

The liquid in the prism was well stirred, and the thermometer read before each adjustment of the prism for minimum deviation. Half the difference between the two sets of eight readings was, of course, the deviation. The readings were made thus in order, as far as possible, to eliminate errors due to the position of the prism, and to slight changes in temperature.

For each solution observations were made with the prism at the temperature of the room, and at 18°. In the former case one set only, as the temperature of the prism did not remain constant, and indeed had usually risen slightly before the one set of readings could be made; in the latter case four sets were made.

The densities of the solutions were determined by means of Dr. Perkins' modification of the Sprengel density tube.* Two of these tubes were used, containing respectively 34·0376 c.c. and 25·1587 c.c. After being filled, the tubes were placed in a water bath at 18° for 20 minutes, the volumes adjusted, and weighed against counterpoises made of the same kind of glass, and having about the same surface areas; the tubes and their counterpoises were wiped carefully with a dry cloth before each weighing, to ensure that, as far as possible, the surfaces were in the same condition as regards moisture. After being weighed, a few more drops of the solutions were introduced into the tubes, which were then replaced in the bath; four determinations being made with each solution.

The water-bath contained a Harcourt gas regulator, and a slow stream of water from the main was run into it; without this the tem-

* 'Chem. Soc. Trans.,' 1884, p. 444.

perature could not be kept down to 18°, the loss of heat by radiation not being sufficient.

The bath was kept stirred by bubbles of air which escaped from an inverted thistle-funnel connected by a rubber tube with a large Woulfe's bottle. A stream of water ran continuously into the bottle, and drove the air out through the rubber tube and thistle funnel. When nearly full of water, the bottle was emptied by a syphon; thus, by the combined action of a continuous stream of water tending to fill the bottle, and the intermittent action of the syphon which emptied it, the water-bath was kept stirred.

The normal and semi-normal solutions were prepared by first making up solutions which contained a gram-molecule in about 800 c.c. or 900 c.c., then determining the strength of these solutions, running the volume required from a burette into a measuring flask, and making the solution up to a known bulk. Distilled water, which had been freed from air by being boiled and allowed to cool under reduced pressure, was used.

The hydrochloric acid was prepared by redistilling the acid sold as "pure redistilled."

The potassium and sodium chloride solutions were prepared from salts sold as "pure recrystallised" (solutions A), and also (solutions B) from salts guaranteed as pure by E. Merck ("für analytische Zwecke"). The "pure recrystallised" potassium chloride contained a good deal of sodium, whilst that obtained from Merck contained hardly any.

The lithium and rubidium chlorides were also purchased from E. Merck, but were of ordinary commercial purity.

The results obtained are given in Table I.

Table I.

Hydrogen Chloride.

Normal solution. 36·78 grams = 1·0091 gram-molecule in 1000 c.c.			Semi-normal solution. 18·24 grams = 0·5005 gram-molecule in 1000 c.c.		
Temp.	Ref. index.	Density.	Temp.	Ref. index.	Density.
15·0°	1·341748	—	16·4°	1·337501	—
18·0°	1·341504	1·01646	18·0°	1·337360	1·00748
	1·341506	1·01647		1·337364	1·00753
	1·341514	1·01647		1·337376	1·00757
	1·341520	1·61647		1·337382	1·00762
	1·341528	—		—	—
Mean..	1·341514	1·01647		1·337370	1·00755

Table I—*continued.*

Lithium Chloride.

Normal solution. 42·40 grams = 0·9982 gram-molecule in 1000 c.c.			Semi-normal solution. 21·161 grams = 0·4981 gram-molecule in 1000 c.c.		
Temp.	Ref. index.	Density.	Temp.	Ref. index.	Density.
15·0°	1·342142	—	14·24°	1·337893	—
18·0°	1·341921	1·02273	18·0°	1·337591	1·01066
	1·341922	1·02274		1·337593	1·01081
	1·341950	1·02277		1·337593	1·01091
	1·341964	1·02279		—	1·01101
Mean..	1·341939	1·02276		1·337592	1·01085

Sodium Chloride.

Normal solution A. 58·16 grams = 0·9940 gram-molecule in 1000 c.c.			Semi-normal solution A. 29·29 grams = 0·5006 gram-molecule in 1000 c.c.		
Temp.	Ref. index.	Density.	Temp.	Ref. index.	Density.
15·8°	1·343317	—	15·6°	1·338420	—
18·0°	1·343090	1·03916	18·0°	1·338198	1·01926
	1·343095	1·03919		1·338200	1·01927
	1·343097	1·03927		1·338200	1·01927
	1·343101	1·03928		1·338203	1·01932
Mean..	1·343096	1·03922		1·338200	1·01928
Normal solution B. 58·39 grams = 0·9980 gram-molecule in 1000 c.c.			Semi-normal solution B. 29·20 grams = 0·4991 gram-molecule in 1000 c.c.		
15·1°	1·343329	—	15·5°	1·338524	—
18·0°	1·343035	1·03916	18·0°	1·338193	1·01913
	1·343063	1·03919		1·338196	1·01916
	1·343066	1·03928		1·338202	1·01916
	1·343075	1·03933		1·338207	1·01926
Mean..	1·343060	1·03924		1·338199	1·01918

Table I—*continued.*

Potassium Chloride.

Normal solution A. 74·20 grams = 0·9949 gram-molecule in 1000 c.c.			Semi-normal solution A. 37·13 grams = 0·4979 gram-molecule in 1000 c.c.		
Temp.	Ref. index.	Density.	Temp.	Ref. index.	Density.
14·0°	1·343380	—	14·4°	1·338460	—
18·0°	1·342926 1·342926 1·342955 —	1·04482 1·04483 1·04485 1·04496	18·0°	1·338135 1·338147 1·338147 1·338153	1·02204 1·02205 1·02207 1·02208
Mean ..	1·342936	1·04487		1·338145	1·02206
Normal solution B. 74·65 grams = 1·0008 gram-molecule in 1000 c.c.			Semi-normal solution B. 37·32 grams = 0·5004 gram-molecule in 1000 c.c.		
15·75°	1·343135	—	15·8°	1·338403	—
18·0°	1·342905 1·342926 1·342934 1·342934	1·04495 1·04503 1·04503 1·04504	18·0°	1·338139 1 338145 1·338146 1·338167	1·02211 1·02212 1·02217 1·02220
Mean ..	1·342925	1·04501		1·338149	1·02215

Rubidium Chloride.

Normal solution. 120·70 grams = 0·9984 gram-molecule in 1000 c.c.			Semi-normal solution. 60·35 grams = 0·4991 gram-molecule in 1000 c.c.		
Temp.	Ref. index.	Density.	Temp.	Ref. index.	Density.
17·2°	1·343958	—	17·4°	1·338634	—
18·0°	1·343840 1·343882 1·343882 —	1·08587 1·08599 1·08604 1·08621	18·0°	1·338577 1·338578 1·338580 1·338601	1·04253 1·04255 1·04258 1·04261
Mean ..	1·343868	1·08603		1·338584	1·04257

The probable errors of the determinations were calculated by the ordinary formula $0{\cdot}674\sqrt{\frac{\Sigma d^2}{n(n-1)}}$; the mean probable error for the

refractive indices was found to be 4 in the sixth place, and for the densities 2 in the fifth place.

The sensitiveness to change of temperature (*i.e.*, the decrease in the value of the index for an increase of 1°) as calculated from the values given in the table is nearly the same for both the normal and semi-normal solutions, the mean value being 0·000095. The value for water as calculated from the values for the indices at 15° and 20°, given by eight different observers, being 0·000080.*

The solutions not being accurately normal or semi-normal, the values for the refractive indices and densities of normal and semi-normal solutions at 18° were calculated from the observed values given in Table I, on the assumption that the differences between the indices and densities of the solutions and those of water are, over this small range, proportional to the weights of salt present in the unit volumes. The values so obtained are given in Table II, those for the potassium and sodium chlorides being the mean values for solutions A and B.

Table II.

	Normal solutions.		Semi-normal solutions.	
	Ref. index.	Density.	Ref. index.	Density.
Hydrogen chloride	1·341438	1·01631	1·337366	1·00754
Lithium chloride	1·341955	1·02280	1·337608	1·01089
Sodium chloride	1·343113	1·03940	1·338201	1·01923
Potassium chloride	1·342955	1·04505	1·338155	1·02214
Rubidium chloride.....	1·343882	1·08616	1·338593	1·04264

Table II shows that both the densities and the refractive indices as a rule increase with the molecular weight, but that in the latter case there is one remarkable exception. The refractive index of potassium chloride is slightly lower than that of sodium chloride.

This fact has already been noticed by Bender† and Borgesius.‡ Bender's observations were made by the hollow prism method, with solutions containing from 0·5 to 4·5 gram-molecules in the litre, and at temperatures between 16° and 21°; he states (p. 92) that the solutions of potassium and sodium chloride had nearly identical refractive indices, those of the latter being slightly the greater, especially with the more concentrated solutions.

Borgesius's observations were made by an interference method, and

* *Conf.* Dufet, 'Recueil de Données Numériques,' p. 87.

† 'Wied. Ann.,' vol. 39, p. 88.

‡ 'Wied. Ann.,' vol. 54, p. 221.

he gives his results as the differences between the indices of his solutions and that of water; his Table I* shows that the values for the potassium chloride solutions were always lower than those for the corresponding sodium chloride solutions.

The refractive index of the solution of a salt clearly depends on the influence both of the solvent and of the salt in solution. In order that the values obtained with different salts should be comparable, it is necessary that the solutions should be similar, *i.e.*, that in unit volume there should be the same weight of solvent and of salt, or the same weight of solvent and weights of salt bearing the same relation to the molecular weights of the salts.

Such is seldom, if ever, the case with aqueous solutions at least. A litre of a normal solution contains, together with the gram-molecular weight of the salt, a weight of water which is different for different salts.

Hence the refractive indices of the solutions of any two salts cannot be taken as a measure of the influence which the several salts exert on the velocity of light, unless it has been shown that the unit volumes of the solutions contain equal weights of water, together with either equal weights of the salts, or weights which bear the same ratio to the molecular weights.

Table III gives the weight in grams of the water contained in 1000 c.c. of the solutions whose refractive indices and densities are given in Table II, obtained by subtracting from the weight of 1000 c.c. of the solution the weight of salt contained in that volume.

Table III.

	Normal solution.	Semi-normal solution.
Water	998·666	
Hydrogen chloride	979·850	989·316
Lithium chloride	980·316	989·653
Sodium chloride	980·885	989·980
Potassium chloride	970·463	984·851
Rubidium chloride	965·272	982·199

The table shows that in equal volumes of the solutions of the chlorides of hydrogen, lithium, and sodium nearly equal weights of water are present, but that such is not the case with the solutions of the two other chlorides.

In a litre of a normal solution of potassium chloride there are about 10 grams, and in a semi-normal solution about 5 grams less water than

* *Loc cit.*, p. 333.

in the corresponding solutions of sodium chloride; hence if the chlorides of the two metals had, when present in solution in molecular proportions, equal powers of retarding the velocity of light, the solution of the potassium salt might be expected to have a lower refractive index than that of the sodium salt, as the unit volume contains, in addition to the salt, less water.

At present we are not in a position to distinguish between the retardation due to the substance in solution and that due to the solvent. If, as a first attempt, we assume (which is, of course, improbable) that the presence of the salt merely causes the water to occupy a greater volume without altering any of its properties other than those which depend on its density, we can calculate approximately the refractive index of the water in the solution.

When two gases are mixed and no mutual action is known to occur, we regard each gas as unchanged except that its density is reduced by the admixture. If a mixture of liquids or the solution of a salt, where no mutual action is known to occur, be similarly regarded, we may consider each of the liquids or the solvent as changed only in respect of its density. If again we attribute the change of refractive index with temperature solely to the change in the density of the liquid, we may make the hypothesis that the effect on the refractive index of water of a change of density is the same when it is expanded by admixture and when it is expanded by rise of temperature. No doubt such a hypothesis, resting upon two hypotheses each of which is improbable, has itself a very small probability, but I have thought it worth while to reckon what the refractive indices of the water in the various solutions would be on this hypothesis. The difference between this value and the observed value of the index of the solution furnishes more probable value for the influence which the salt may be supposed to exert on the velocity of light than that obtained by subtracting the index of water from the index of the solution at the same temperature.

From Landolt and Börnstein's tables the temperatures were ascertained at which water has the same density as that contained in the various solutions, and then from the same tables the refractive index of water for these temperatures and densities.

The values for the differences between the observed indices of the various solutions and the index of water at 18° are given in the second and third columns of Table IV, and the differences between the observed indices and the values for the indices of water, calculated on the above assumption, in the fourth and fifth columns.

The table shows that the differences between the refractive indices of the solutions, and that of water at 18° increase with the molecular weights of the salts in solution, except in the case of potassium chloride, but if the differences are calculated on the assumption that *the refractive* index of the water in the solutions is less than that of

Table IV.

	Normal solution.	Semi-normal solution.	Normal solution.	Semi-normal solution.
Hydrogen chloride...	0·00831	0·00423	0·01551	0·00792
Lithium chloride ...	0·00882	0·00448	0·01583	0·00804
Sodium chloride.....	0·00998	0·00507	0·01679	0·00854
Potassium chloride...	0·00982	0·00502	0·02047	0·01036
Rubidium chloride ...	0·01075	0·00546	0·02328	0·01172

water in its ordinary condition, then potassium chloride no longer forms an exception, and the differences increase with the molecular weights of the salts; the increase, however, does not bear any apparent relation to the increase in the molecular weights.

The differences for the normal solutions, calculated in both ways, are slightly less than double those for the semi-normal.

The object of these experiments was the determination of the refractive indices and densities of normal and semi-normal aqueous solutions of hydrogen chloride and the chlorides of the alkalis at a uniform temperature; the results obtained at 18° are set forth in Table I, the indices being given to six places of decimals and the densities to five places.

In Table II the results corrected for small errors in the strengths of the solutions are given. The table shows that both the densities and the refractive indices increase with the molecular weight of the substance in solution, except in the case of the refractive index of potassium chloride, which is slightly lower than that of sodium chloride.

Table III gives the weight of water contained in 1000 c.c. of each of the solutions, and Table IV the differences between the refractive indices of the solutions, and the refractive index of water under ordinary conditions, and also the differences between the refractive indices of the solutions and the calculated indices of the water contained in the solutions.

[*Note.*—From the data given in Table I, it is not possible to draw any satisfactory conclusions as to the sensitiveness of the different solutions. The values for the indices at temperatures other than 18°, rest on single sets of observations, and in some cases there was not sufficient difference between the temperature at which these observations were made and 18°, to enable the rate of change of the index to be determined at all accurately.

I have, therefore, made some further observations with the same solutions of hydrogen chloride and the chlorides of lithium, sodium,

and potassium, and also with water. In both the rubidium solutions a mould had developed, and therefore no observations were made with these two solutions.

The observations were made in the same way as those already described, except that (1) the gas regulator was removed from the water bath containing the coil, and the temperature was allowed to rise, and (2) all the measurements were made with the prism in one position.

The temperature of the prism rose slowly, about 2° in an hour; the prism was not reversed in order to avoid exposing its surfaces to the cooling action of the air of the room. The object of the experiments being to ascertain the relative values of the index of a solution at different temperatures, and not the absolute value at any particular temperature, the result would not be affected by any small error due to the observations having been made with the prism in one position of minimum deviation and in one position only.

The values obtained were plotted, one unit in the fifth place being represented by 1 mm., and the rate of change, the "sensitiveness," ascertained from the plotting.

The results are given in the table: in the second and fifth columns the temperatures between which the observations were made, in the third and sixth the number of observations, and in the fourth and seventh the decrease of the index for an increase of 1°.

	Normal solution.			Semi-normal solution.		
Water......	11·10°—20·85°	6	0·000078			
Hydrogen chloride...	10·85 —18·40	7	0·000077	10·05°—17·45°	7	0·000072
Lithium chloride...	12·10 —21·20	8	0·000083	9·75 —18·65	6	0·000073
Sodium chloride, B.	12·25 —21·10	13	0·000109	10·00 —19·5	6	0·000087
Potassium chloride, B.	9·15 —19·70	7	0·000095	9·35 —19·55	8	0·000085

The table shows that between 10° and 20° the "sensitiveness" of the normal solutions is greater than that of the semi-normal solutions; and further, that it increases with the increase of the refractive index of the solution.

The value for water between 10° and 20° calculated from the values given by Dufet* is 0·000071, or slightly less than that obtained in these experiments.—December 31, 1898.]

* *Loc. cit.*

"Sets of Operations in Relation to Groups of Finite Order." By A. N. WHITEHEAD, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor A. R. FORSYTH, F.R.S.

Received January 19,—Read February 2, 1899.

(Abstract.)

Introduction.

The present paper is concerned with the Theory of Groups of Finite Orders. The more general object of the paper is to place this theory in relation to a special algebra of the type considered in the general theory of Universal Algebra. This special algebra, which may be called the Algebra of Groups of Finite Order, has many affinities to the Algebra of Symbolic Logic; and a comparison of it with this algebra is given in the last section of this paper.

Mathematicians are accustomed in the study of quaternions to the idea of a vector symbol being considered from two points of view according to circumstances, namely, either as a geometrical entity, or as a symbol expressive of the operation of modifying some geometrical entity into another geometrical entity. Now from the point of view of this paper it is natural to abandon the idea of a group of N operations $S_0, S_1, \ldots\ldots S_{N-1}$ on some unspecified object, as being an idea which, however vaguely, appertains to a special interpretation of the symbols. The N symbols $S_0, S_1, \ldots\ldots S_{N-1}$ are to be considered, as in the similar case of quaternions, primarily as N distinct objects. When two of these objects are multiplied, as in $S_q S_r$, then a third object of the group, such as S_p, is produced; and in reference to this multiplication one of the symbols, say S_q, may be looked on as an operation on S_r modifying it into S_p. But this is not the sense in which the symbols are usually called operations in the Theory of Groups. However, in order not to disturb the well understood nomenclature of the subject, the N objects $S_0, S_1, \ldots\ldots S_{N-1}$ will always be called the fundamental operations, or, more shortly, the operations. But the word operation can simply be regarded as a name for the objects represented by these N symbols.

These N symbols are considered to be capable of addition according to the law

$$S + S = S.$$

This is the well known law of addition in Symbolic Logic, and the introduction of numerical symbols as factors is thereby avoided.

The sum of a selection of the N fundamental operations, such as $S_p + S_q + S_r + S_t$, is called a set. If a set obeys certain special conditions it is called a group. The sum of the whole number (N) of fundamental

operations, namely, $S_0 + S_1 + \ldots\ldots + S_{N-1}$, obeys these conditions. This sum is called the complete group, and all other groups are its sub-groups.

The first six sections of this paper are devoted to the detailed establishment of this purely algebraic view of the subject. At times the modification in treatment from that adopted in the standard treatises on the subject, such as Burnside's 'Theory of Groups of Finite Orders,' is slight. Where the modification would be of no sufficient interest it has been simply omitted, and the theorems when wanted have been assumed as part of the general knowledge of the subject. Only so much reasoning has been given as will establish the principles of the Algebra of Groups of Finite Order, viewed as an algebra independent of any interpretation, however vague.

The more special object of this paper follows directly from the changed point of view from which the Theory of Groups is here regarded. The idea of the group is no longer so absorbing; the set takes its place as the fundamental general entity which has to be investigated. A group is a special type of set. Accordingly in this paper some of the general properties of sets are investigated. A set of operations has numerous groups associated with it, and these groups have many relations with each other which this paper cannot pretend to have exhausted. The fundamental idea of this part of the paper (*cf.* § 7) is the formation from a set H of an unending series of other sets, here called the successive powers of H, and in the notation of the algebra written H^2, H^3,...... This series is called the power sequence of H. Any group which contains H also contains its power sequence. The power sequence is proved to have a periodic property (*cf.* § 9) which introduces a curious analogy to recurring decimals. This periodicity is the foundation of the rest of the paper. It governs the relations to each other of the various allied groups and sets. The periodicity is expressed by an equation of the form

$$H^{n+sm+q} = H^{n+q},$$

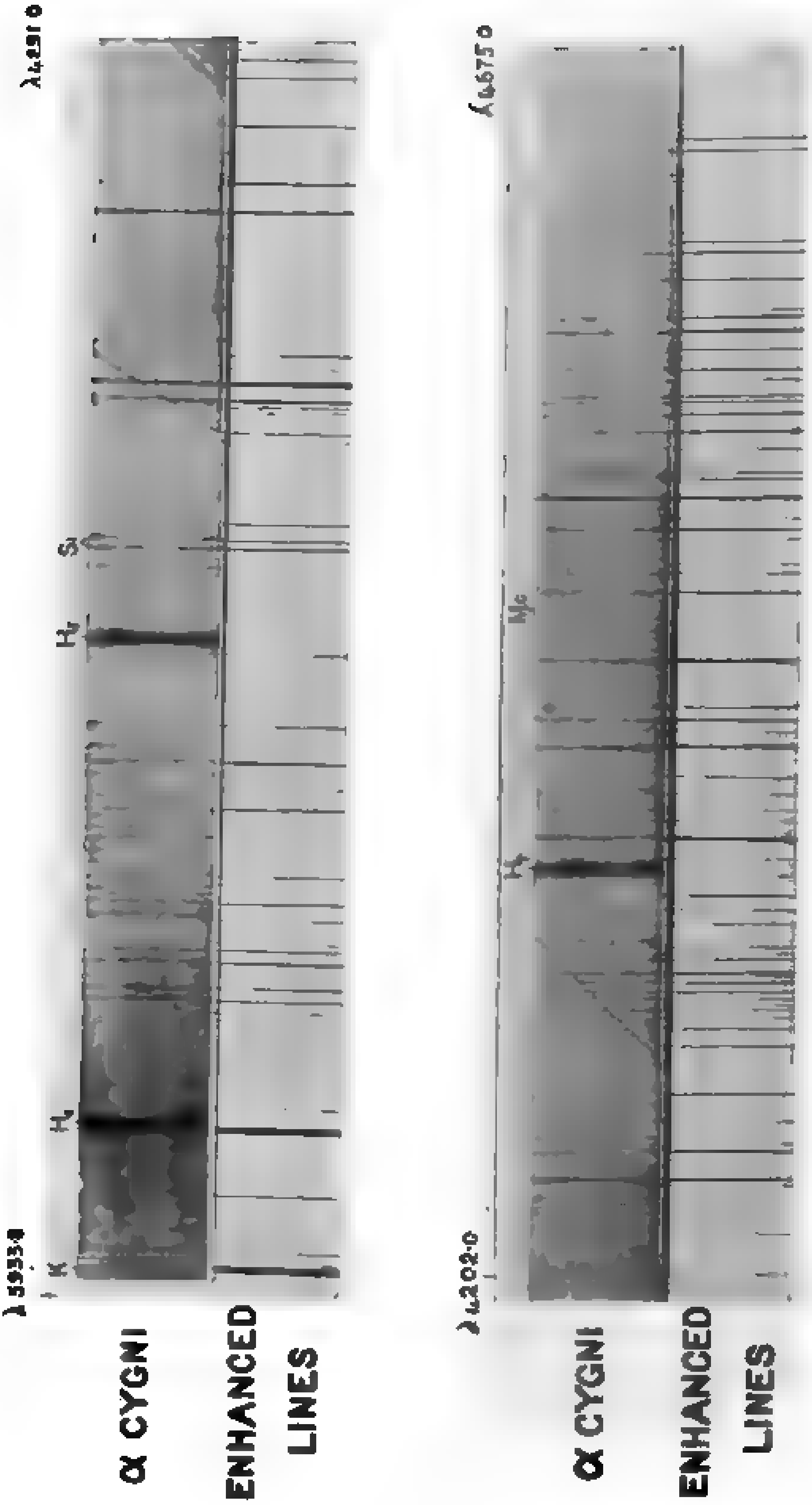
where m is called the period of H, and n the characteristic, and s and q are any integers including zero. The number of theorems relating to m is very large.

"Note on the Enhanced Lines in the Spectrum of *a* Cygni." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received January 20—Read February 2, 1899.

(PLATE 6.)

When engaged in the classification of stars, according to their photographic spectra, in 1893* I came across two sets of lines of

* 'Phil. Trans.,' A, vol. 184, p. 675.


λ4481.0
λ3933.0
α CYGNI
ENHANCED LINES
λ4675.0
λ4202.0
α CYGNI
ENHANCED LINES

unknown origin, one in the hottest stars, the other in stars of intermediate temperature.

After the discovery of a terrestrial source of helium by Professor Ramsay, I showed in a series of seven notes communicated to the Royal Society,* May—September, 1895, that the cleveite gases, which I obtained by the process of distillation, accounted to a very great extent for the first set.

In 1897 in a series of three communications to the Royal Society,† I pointed out that some of the other set of unknown lines in the stars of intermediate temperature, taking α Cygni as an example, were due to the enhanced spark lines of iron and other metals, the arc lines being almost entirely absent.

During the last year, this research has been continued; and latterly, by the kindness of Mr. Hugh Spottiswoode, the photographs of the enhanced lines have been obtained by the use of the large induction coil, formerly belonging to Dr. Spottiswoode, P.R.S. I am anxious to express here my deep obligation to Mr. Hugh Spottiswoode for the loan of such a magnificent addition to our instrumental aids.

The spark obtained by means of the Spottiswoode coil, is so luminous that higher dispersions than those formerly employed can be effectively used, and in consequence of this, the detection of the enhanced lines becomes more easy; their number therefore has been considerably increased.

The observations have already been mapped for the following substances:—Fe, Mg, Ca, Si, Sr, Va, Ti, Ni, Mn, Cr, Co, Cu.

In the accompanying photograph, a comparison is shown between the lines of α Cygni and the enhanced lines of the above substances thrown together. The extraordinary number of coincidences is seen at a glance. The facts are as follows:—

The number of lines measured in the spectrum of α Cygni at Kensington between λ3798·1 and λ4861·6 is	307
Of these the number which approximately coincides with the enhanced metallic lines so far observed is........................	120
The number of lines (excluding the hydrogen series) in α Cygni of intensity over 4 (the maximum being represented by 10) is	40
Of this number, the coincidences with enhanced metallic lines with the dispersion employed amount to	38

I shall deal in a subsequent communication, when the enquiry has reached a further stage, with the details for each element.

* 1st Note, 'Roy. Soc. Proc.,' vol. 58, p. 67; 2nd, *ibid.*, vol. 58, p. 113; 3rd, *ibid.*, vol. 58, p. 116; 4th, *ibid.*, vol. 58, p. 192; 5th, *ibid.*, vol. 58, p. 193; 6th, *ibid.*, vol. 59, p. 4; 7th, *ibid.*, vol. 59, p. 342.

† 'Roy. Soc. Proc.,' vol. 60, p. 475; *ibid.*, vol. 61, p. 148; *ibid.*, vol. 61, p. 441.

The lines of the stars of intermediate temperature, like α Cygni, have long been recognised by the Harvard observers as well as by myself as presenting great difficulties.

In 1893 I wrote as follows* :—"With the exception of the K line, the lines of hydrogen aud the high temperature line of Mg at λ4481, all the lines may be said to be at present of unknown origin. Some of the lines fall near lines of iron, but the absence of the strongest lines indicates that the close coincidences are probably accidental."

In the Harvard 'Spectra of Bright Stars' 1897, p. 5, the following words occur, relating to the same stars :—

"This system of lines should perhaps be regarded as forming a separate class, as in the case of the Orion lines, and should not be described as 'metallic,' as has just been done in the absence of any more distinctive name."

From the fact that these unknown lines have now been traced to a "proto-metallic" origin, as effectively as the unknown lines of the hottest stars have been traced to helium and asterium, we may expect that the consequences of this determination in relation to stellar classification and other connected matters, will be very far reaching. At present I am using this new spectrum consisting of enhanced lines as an explorer, in relation to some further details of stellar classification having special reference to stars of Groups III and IV in which bright as well as dark lines occur.

"On the Effects of Strain on the Thermo-Electric Qualities of Metals." By MAGNUS MACLEAN, M.A., D.Sc. Communicated by Lord KELVIN, F.R.S. Received January 23,—Read February 2, 1899.

1. Seebeck† discovered the great effect that hardness, or softness, or crystalline structure, has on the thermo-electric properties of metals. Magnus made a number of experiments by winding a hard drawn wire on a reel. Parts of this wire were softened and annealed. When heat was applied to the parts of the wire which were between unannealed and annealed, a thermo-electric current was obtained. In this way Magnus found that the current passed from soft to hard through the hot junction for silver, steel, cadmium, copper, gold, and platinum; and that it passed from hard to soft through the hot junction for German silver, zinc, tin, and iron.

2. Lord Kelvin describes in vol. 2 of his 'Mathematical and Physical Papers' a number of qualitative experiments to determine the direction

* 'Phil. Trans.,' A, vol. 184, p. 694.

† 'Pogg. Ann.,' 1826.

of thermo-electric currents in the same metal when one part of it is left unstrained, and the other is—

(1) Permanently affected by application and removal of longitudinal stress;

(2) Permanently affected by application and removal of lateral pressure;

(3) Under a longitudinal stress (*a*) within its limits of elasticity, and (*b*) beyond its limits of elasticity;

(4) Hardened by twisting;

(5) Annealed.

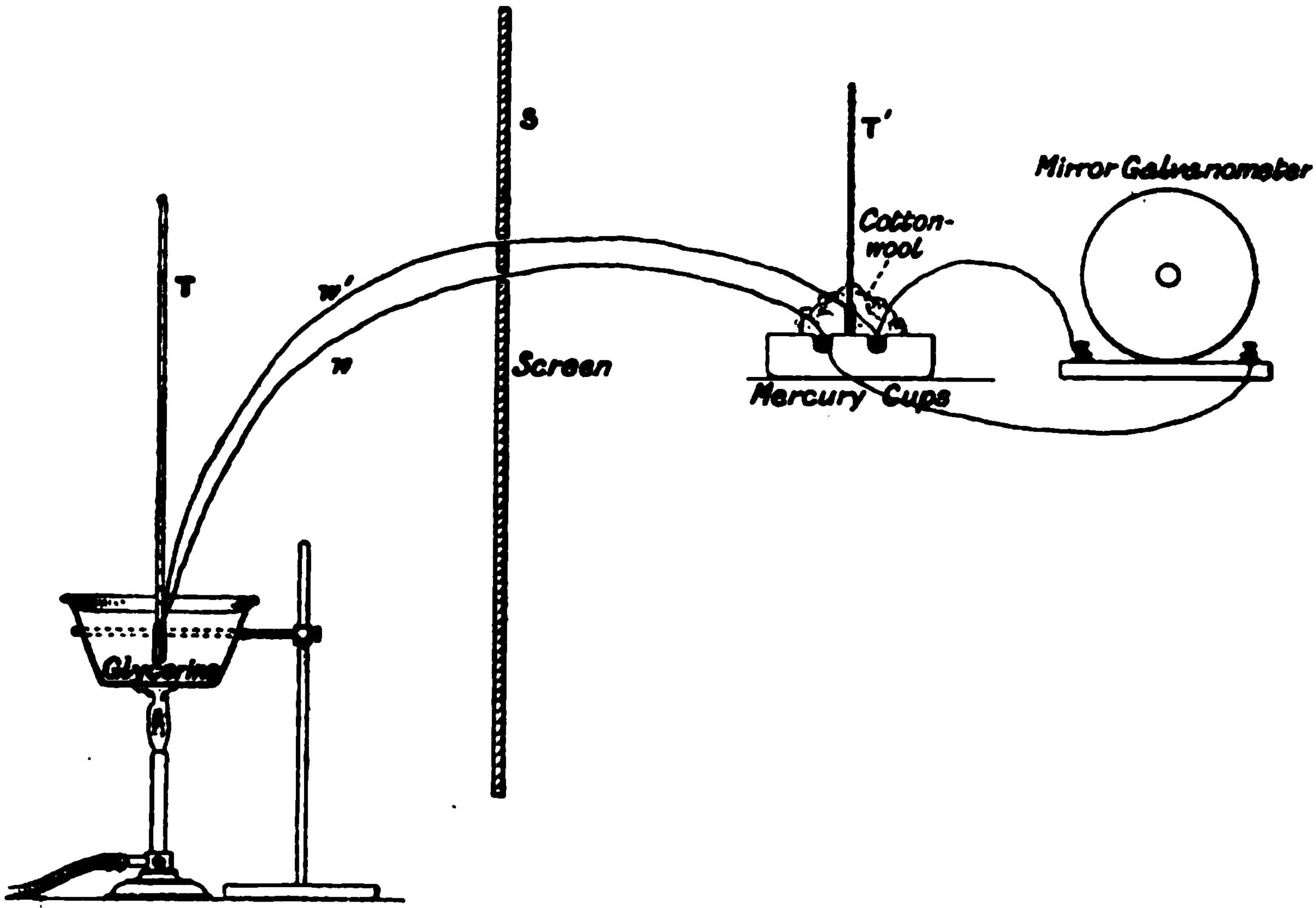
3. He showed that for iron and copper permanent longitudinal extension gave the same effect as permanent lateral contraction; and that this effect for both was opposite to that experienced by them when under a stress which caused a temporary strain. Thus for a copper wire under a longitudinal stress the current was from the strained copper to the free copper across the hot junction, and the magnitude of the current increased with the increase of the longitudinal stress. If the stress were removed and the wire left with a permanent strain, the current was now from the free copper to the strained copper through the hot junction. Similar results were got with iron, only the direction of the current was in each case opposite to the direction of the current in the corresponding case for copper. The highest temperature used in these experiments was about 100° C.

4. A summary of Lord Kelvin's results is given on pages 296 and 297 of vol. 2 of his 'Mathematical and Physical Papers.' His results for copper are given, for example, in the following table:—

Conductor.	Condition of strained conductor. Direction of current through hot junction is from 1 to 2.
Copper.........	1. Under longitudinal traction. 2. Free.
„	1. Soft. 2. Permanently elongated by longitudinal traction, and left free from stress.
”	1. Soft. 2. Hammered transversely.
Round copper wire	1. Annealed after being made brittle by twisting. 2. Made brittle by twisting.
” ”	1. Annealed. 2. Suddenly cooled.

5. To determine the *magnitude* of the thermo-electric effects obtained from any one metal, strained and unstrained, was the object I had in

view when I started these experiments. The arrangement is shown diagrammatically below. One junction of the wires was kept in a



glycerine bath which could be heated by a Bunsen burner. This junction was tied by a fine copper wire to the bulb of a thermometer T. The other ends of the wires were joined to short copper wires which served as terminals of the low resistance galvanometer used in the experiments. These junctions were wrapped in paraffin paper or cotton wool which contained the bulb of a thermometer T′, reading half degrees from 0°C. to 25° C. A paper screen S was hanging vertically between the Bunsen burner and the thermometer T′ and the galvanometer to prevent any heat from the flame reaching the rest of the circuit by radiation. These precautions were taken to make certain that all junctions, except the hot junction, would be at the same temperature.

6. The constant of the galvanometer was determined by joining a Daniell cell in circuit with the galvanometer and with a resistance of over 30,000 ohms. The electromotive force of the Daniell cell was compared with a standard Clark cell by means of a quadrant electrometer. In this way the current through the galvanometer per division deflection on the scale was determined. The sensitiveness, in preliminary experiments, was 0·612 mikroampere per division. But by different arrangements of the controlling magnets, the sensitiveness of the galvanometer as now used is 0·09 mikroampere per division. The resistance of the galvanometer, at 15° C.—the average temperature of the laboratory during the experiments—is 1·5 ohms. Thus the electro-

motive force at the terminals of the galvanometer as now used is 0·135 mikrovolt per division.

7. A considerable lag was found in the thermometer readings, and the following method was adopted to get rid of this effect. The hot junction was heated very slowly through a small range (5° C. or 10° C.) and then the Bunsen burner was drawn slightly aside so as to give approximately as much heat to the vessel which contained the glycerine as it lost by radiation. The thermometer, T, and the spot of light on the scale were simultaneously observed, and when both were seen to be steady, the readings were noted. The circuit was then immediately broken and readings taken of the galvanometer zero* and the thermometer T′.

Very often the glycerine was allowed to cool slowly, and by means of a small Bunsen flame the temperature was kept steady for a short time and readings taken. If the same precautions were taken as are described in the previous paragraph, the same deflections from zero were got for the same difference of temperature between the hot and cold junctions.

8. The metals so far tried are :—

(1) Copper wire from Messrs. Johnson and Matthey. This was pure electrotype copper wire with no impurity detected except an unweighable trace of iron.

(2) Copper wire, ordinary commercial, from Messrs. Johnson and Matthey. This was analysed† in the chemical laboratory of the University, and was found to contain :—

* It was found necessary to take the zero immediately after each reading as the zero was by no means constant. It was thought at first that the change of zero was mainly due to the suspending fibre of the mirror in the galvanometer, but a new plug and fibre did not lessen the variations of the zero during an experiment. It is most likely due to the general laboratory experiments going on simultaneously, which involve the moving of apparatus and the walking about of students with knives and keys in their pockets in the near vicinity of the galvanometer. For example, my own pocket knife at the distance of the scale from the galvanometer (a metre) gives a deflection of 10 scale divisions. The galvanometer is at a distance of 11·74 metres from the dynamo used in the electric light installation of the Physical Laboratory, the two being separated by a stone wall. The stopping or the starting of the dynamo altered the metallic zero of the galvanometer by 100 divisions. The constant of the galvanometer was tested both when the dynamo was running and when the dynamo was not running. Practically it was the same on both occasions. Nearly all the experiments were done when the dynamo was running.

† All the chemical analyses stated in this paper were given to me by Mr. Anderson, of the Chemical Laboratory of this University.

Copper	99·4	per cent.
Arsenic	0·44	„
Lead	0·08	„
Bismuth	trace	
	99·92	„

(3) Copper wires, used for alloying with gold and silver, from Messrs. Johnson and Matthey. This also was analysed and it contained 99·85 per cent. of copper.

(4) Copper wire from Glover. Chemical analysis showed that it contained 98·35 per cent. of copper.

(5) Copper wire of Glover's manufacture and supposed to be soft and to have a very high conductivity. It contained 99·08 per cent. of copper and 0·22 per cent. of lead.

(6) Copper wire used in laboratory experiments. It contained 98·51 per cent. of copper.

(7) Lead wire, commercial. It contained 98·9 per cent. of lead.

(8) Lead wire, pure.* It contained 98·97 per cent. of lead.

(9) Platinoid wire obtained from Messrs. Glover.

(10) German silver wire „ „ „

(11) Reostene† „ „ „ „

(12) Manganin „ „ „ „

9. The size of the wire used, except for (5) (7) (8) above, was about No. 18 standard gauge. A piece of the wire was taken and drawn through a draw plate till it was reduced to about No. 24 standard gauge. This process of wire drawing subjects the wire to longitudinal extension and to lateral compression. Lord Kelvin in his experiments ('Mathematical and Physical Papers,' vol. 2 and section 3 above) showed that thermo-electric differences were in the same direction for longitudinal extension and transverse compression. For drawn and undrawn wires the direction of the current through the hot junction is from undrawn to drawn for copper, reostene, and lead, and from drawn to undrawn for platinoid, German silver, and manganin. The magnitude of the current per degree difference of temperature is given in the following table.

* These specimens of commercial and pure lead wires were obtained from Messrs. Baird and Tatlock of Glasgow. Other specimens have been ordered elsewhere for a fresh determination.

† Reostene belongs to the nickel steel group, with certain other metals as an alloy. Messrs. Glover and Co. could not give me particulars regarding it, or regarding manganin, which is composed of copper, tin, and manganese.

Conductor.	Condition of conductor. Direction of current through hot junction is from 1 to 2.	Current in mikro-ampere per degree up to 100° C.
Copper, pure electrotype, Messrs. Johnson and Matthey	1 undrawn 2 drawn	0·0057
Copper, commercial, Messrs. Johnson and Matthey	1 undrawn 2 drawn	0·0279
Copper, used for gold alloy, Messrs. Johnson and Matthey	1 undrawn 2 drawn	0·0104
Copper, commercial, Glover	1 undrawn 2 drawn	0·0068
Copper, soft, high conductivity, Glover	1 undrawn 2 drawn	0·031
Copper, laboratory	1 undrawn 2 drawn	0 0435
Lead, pure	1 undrawn 2 drawn	0·0087
Lead, commercial	1 undrawn 2 drawn	0·0126
Reostene, Glover	1 undrawn 2 drawn	0·173
Platinoid „	1 drawn 2 undrawn	0·533
German silver, Glover	1 drawn 2 undrawn	0·105
Manganin, Glover	1 drawn 2 undrawn	0·031

10. The resistances of all the undrawn wires were carefully determined by the usual bridge method.

The specific gravities and cross sections of both the undrawn and drawn wires were also determined by weighing known lengths in air and in water. The values are given in the following table. It will be noted that the specific gravity of drawn copper, of drawn commercial lead, of drawn platinoid, and of drawn manganin, is greater* than for the corresponding undrawn wires; that the specific gravity of undrawn and drawn German silver wire is the same; and that the specific gravity of drawn reostene wire and of drawn pure lead wire is less than that of the undrawn.

* Average about a half per cent.

Metal.	Cross section of undrawn and drawn wires.	Specific gravity of undrawn and drawn wires.	Resistance of the undrawn wires, in C.G.S. units, at the temperature stated. Per cubic centimetre.	Per centimetre long, weighing a gram.
	sq. cm.			
Copper, Johnson and Matthey, No. 1	0·01172 0·00218	8·9607 8·996	1680	15050 at 13° C.
Copper, Johnson and Matthey, No. 2	0·01171 0·002233	8·856 8·897	4665	41310 at 13·5° C.
Copper, Johnson and Matthey, No. 3	0·01174 0·002086	8·963 9·05	1859	16660 at 13·5° C.
Copper, hard, Glover	0·0116 0·002018	8·923 8·982	1760	15700 at 17° C.
" soft "	0·006506 0·002421	8·898 9·074	1681	14960 at 17° C.
" laboratory	0·01192 0·002458	8·832 8·908	1764	15580 at 17·5° C.
Lead, pure........	0·01145 0·002448	11·25 11·15	20770	233600 at 14·5° C.
" commercial ..	0·01181 0·002404	11·14 11·23	22100	246200 at 15° C.
Reostene	0·01142 0·002531	7·862 7·667	77230	607100 at 17·2° C.
Platinoid	0·01114 0·002316	8·74 8·78	40570	354600 at 17·3° C.
German silver	0·0116 0·002295	8·756 8·755	32340	283100 at 17·5° C.
Manganin.........	0·0116 0·002443	8·515 8·58	41040	349400 at 17·2° C.

11. The resistances of the drawn copper and manganin wires were compared with the resistances of the corresponding undrawn copper and manganin wires by the fall of potential method, and it was found that the resistances of the drawn wires (for the same length and cross section) were slightly greater* than that of the undrawn wires. In calculating the total resistance in the circuit external to the galvanometer, this increase of resistance in the drawn wire is not taken into account. The circuit consisted of 60 cm. of the undrawn wire, and 60 cm. of the drawn wire, together with the low resistance galvanometer. By multiplying the current per division, given in the table of Section 9, by the total resistance of the circuit, the thermo-electric difference per degree between drawn and undrawn wires is found. The numbers are given in the following table:—

* Less than 1 per cent.

Metal.	Resistance in international ohms of 60 cm. of wire.		Total resistance external to galvanometer.	Total resistance in circuit.	Thermo-electric difference in mikrovolt per degree of difference of temperature.
	Undrawn.	Drawn.			
Copper, Johnson and Matthey, No. 1 ..	0·0086	0·0462	0·0548	1·555	0·0089
Ditto, No. 2 ..	0·0239	0·1254	0·1493	1·649	0·0460
Ditto, No. 3	0·0095	0·0536	0·0631	1·563	0·0163
Copper, hard, Glover	0·0091	0·0523	0·0614	1·561	0·0106
" soft, "	0·0155	0·0417	0·0572	1·557	0·0483
" laboratory ..	0·0089	0·0431	0·0520	1·552	0·0675
Lead, pure.........	0·1088	0·5043	0·613	2·113	0·0184
" commercial ...	0·1123	0·5517	0·664	2·164	0·0273
Reostene...........	0·4058	1·831	2·237	3·737	0·6465
Platinoid...........	0·2186	1·052	1·271	2·771	1·477
German silver	0·1673	0·845	1·013	2·513	0·2638
Manganin..........	0·212	1·008	1·220	2·720	0·0843

12. The copper wires numbered 1, 2, 3, of Messrs. Johnson and Matthey were also tried undrawn.

Conductors.	Direction of current through hot junction.	Current in mikroampere per division up to 100° C.	Difference of potential per degree in mikrovolt.
Copper, Messrs. Johnson and and Matthey, Nos. 1 and 2	2 to 1	1·099	1·667
Ditto, Nos. 2 and 3	2 to 3	0·48	0·743
Ditto, Nos. 3 and 1	3 to 1	0·62	0·942

13. The effect of hardening by twisting has been partially tried. Thus two pieces of laboratory copper wire were taken, and one was in successive experiments twisted 1 turn, 3 turns, 5 turns, 7 turns, 8½ turns per cm. The wire with 8½ turns per cm. got quite brittle, and broke when an attempt was made to put more twists into it. The twisted wire was then heated red hot by an electric current, and allowed to cool. This partially annealed it.

The results are given in the following table :—

Number of turns in twisted wire per centimetre.	Thermo-electric difference between untwisted and twisted copper wire in mikrovolt per degree.
1	0·0054
3	0·0223
5	0·0262
7	0·0419
8·5	0·0594
8·5 and partially annealed	0·0345

14. The effects of twist on the drawn copper wire were also tried, and it was found that 1, 2, 3 turns per cm. in the drawn wire slightly diminished the thermo-electric difference obtained between the undrawn wire and the drawn wire; but that 4 and 5 turns per cm. in the drawn wire gave the same thermo-electric difference as was found between the undrawn wire and the untwisted drawn wire.

15. The drawn and twisted copper wire was annealed by putting a gradually increasing current through it till it got red-hot, and then, without breaking the circuit, the current was gradually reduced till the wire was at the temperature of the laboratory. Trying it in this condition along with the undrawn and untwisted copper wire, the current through the hot junction was found to be reversed, being from the drawn twisted and annealed wire to the undrawn wire. The thermo-electric difference was 0·0081 mikrovolt per degree.

16. Similar experiments on platinoid wires as those described in Section 14 on copper wires gave similar results. Thus 1, 2, 3 turns per cm. in the drawn platinoid wire diminished the thermo-electric difference obtained between the drawn wire and the undrawn wire; but 4 and 5 turns per cm. in the drawn wire gave the same thermo-electric difference (1·477 mikrovolts per degree) as was found between the untwisted drawn wire and the undrawn wire.

17. The drawn and twisted platinoid wire was partially annealed, and the thermo-electric difference between it and the undrawn platinoid wire was thereby reduced from 1·477 mikrovolts per degree to 0·567 mikrovolt per degree.

18. A beginning has been made of determining the thermo-electric differences between free wires and wires previously permanently elongated 1, 2, 3, &c. per cent. by a simple longitudinal stress; also wires while (*a*) under stress, stretching them within their limits of elasticity; and (*b*) under stress, stretching them beyond their limits of elasticity. I hope to be able soon to communicate the results to the Society.

"The Constitution of the Electric Spark." By ARTHUR SCHUSTER, F.R.S., and G. HEMSALECH. Received January 24,—Read February 2, 1899.

(Abstract.)

When an electric spark passes between metallic electrodes, the spectrum of the metal appears, not only in immediate contact with the electrodes, but stretches often across, from pole to pole. It follows that during the short time of the duration of the spark, the metal vapours must be able to diffuse through measurable distances.

The following investigation was undertaken primarily to measure this velocity of diffusion with the special view of comparing different metals, and different lines of the same metal.

Feddersen published, in the year 1862, an interesting research, in which photographs of sparks passing between different metal poles are taken after reflection from a rotating mirror. He could from his experiments draw some conclusions which have a bearing on the subject, but it was necessary for our purpose that the light should also be sent through a spectroscope, so as to distinguish between the luminous particles of air and those of the metal poles.

The method of the rotating mirror tried during the course of several years in various forms by one of us, did not prove successful. On the other hand good results were obtained at once on trying the method used by Professor Dixon, in his researches on explosive waves. This method consists in fixing a photographic film round the rim of a rotating wheel. All that is necessary for its success is to have sparks so powerful that each single one gives a good impression of its spectrum on the film. Were the sparks absolutely instantaneous, the images taken on the rotating wheel would be identical with those developed on a stationary plate, but on trial this is found not to be the case. The metal lines are found to be inclined and curved when the wheel rotates, and their inclination serves to measure the rate of diffusion of the metallic particles. The air lines, on the other hand, remain straight, though slightly widened.

To avoid the tendency of the film to fly off the wheel when fixed round its rim, as in the original form of the apparatus, a spinning disc was constructed for us by the Cambridge Scientific Instrument Company. The film is placed flat against the disc, and is kept in place by a second smaller disc, which can be screwed lightly to the first. The diameters of the two discs are 33 and 22·2 cm., the photographs being taken in the annular space of 10·8 cm., left uncovered by the smaller disc. An electric motor drives the disc, and we have obtained velocities of 170 turns per second, though in our experiments the number of

revolutions was generally about 120, giving a linear velocity of about 100 metres/second for that part of the film on which the photograph was taken.

The electric discharges were obtained from a battery of six Leyden jars, having a total capacity of 0·033 microfarad, and being charged from an induction machine constructed for us by Mr. H. C. Wimshurst. This machine has twelve plates of 62 cm. diameter, and gives sparks which are 13 inches long. The electrodes were, as a rule, placed 1 cm. apart, and an image of the spark was projected on the slit of the spectroscope, the distance of the slit from the electrodes being equal to four times the focal length of the projecting lens, so that the image was equal in size to the spark. The prism used was made by Steinheil, and had a refracting angle of 60°.

We may now pass to the description of the results obtained when the spectrum of a single spark is taken on a moving film. A preliminary trial with various metallic electrodes had shown us that the sharpest results were obtained with zinc, and we therefore chose that metal for our first investigation. The principal lines of zinc as they appear on our photographs are the double line, the least refrangible of the two having a wave-length 4924·8, and the blue triplet, the wave-length of the leading line being 4810·7. All the lines are curved on the photographs taken with the spinning disc, but the displacements, especially near the poles, are subject to considerable variations. This is probably due to the fact that the path of the metallic particles is not always straight, and, if straight, its image does not necessarily coincide with the slit. A very slight error in measurement will also affect the results considerably when the total displacement measured is small. Our results do not for this reason allow us at present to give any opinion as to the maximum velocity of the particles near the pole; but if these are considerable, they drop down very quickly to speeds which, in the case of zinc, are not far off 500 metres/second.

We have adopted two methods of comparison between different photographs. We have in the first place measured the displacements at a number of nearly equidistant points, and from these measurements we have deduced the time taken for a metallic molecule to pass from the pole to a point 2 mm. away from it. If this method could be applied in every case, it would form a rational and consistent basis of comparison. But the curved lines which are to be measured are often very diffuse near the pole; this, and the continuous spectrum, may render it impossible to obtain satisfactory measurements at that point. In order not to have to reject unnecessarily a large number of measurements because the spectrum near the pole was indistinct, we have adopted another method, which, though less rational than the first, is found to give consistent results. From all our measurements we may *deduce certain* figures for the molecular velocities at different and

generally equidistant points on the photographs, and may take the average of all these figures as the mean velocity of the particle. In the tables given in the paper, V_1 always refers to the mean velocity between the pole and a point 2 mm. away from it, while V_2 refers to the average velocity taken for different distances, as just explained. The influence of change of capacity and change in the length of the spark was investigated in the case of zinc, and the following table exhibits the results. As the zinc lines are sharp near the pole, the first of the above methods of measurement could be applied.

Table I.—Average Velocity (V_1) in metres/second of Zinc Molecules.

Sparking distance.	Wave-length.	Number of jars.		
		2.	4.	6
cm.				
0·51	4925	814	556	416
	4811	1014	668	529
1·03	4925	400	499	415
	4811	501	548	545
1·54	4925	723	1061	435 ?
	4811	1210	1526	492 ?

The first striking result to be deduced from the table is the uniformly higher velocity deduced from the double line 4925, as compared with that found when one of the lines of the triplet is measured; for we have ascertained that the two first lines of the triplet are always displaced by the same amount, and the third is so much mixed up with the air lines in its neighbourhood that it cannot be measured. It was one of the objects of the investigation to detect, if possible, differences of this kind, which might be accounted for by the fact that the molecules producing different lines of the same spectrum have not necessarily the same mass. We nevertheless hesitate to ascribe the smaller apparent velocity derived from $\lambda = 4925$ to this reason. This line, as has been mentioned, is one component of a double line, and the doublet is not resolved on the photographs taken with the moving film. Near the pole where the light is strong, the edge of the least refrangible component of the doublet would be considered to be the least refrangible edge of the doublet; but near the centre of the spark the light is weaker, and the lines, owing to the motion of the wheel, are drawn out towards the violet. The most intense portion of the image will here be that part where the two lines are superposed, and in wishing to set the cross wire on the edge of the line, we should be

tempted to set it on the edge of the *most* refrangible component. There is reason to believe that this is the cause of the greater deflection of the double line, and the photographs show some signs that if this source of error is eliminated, the molecule giving out the double line moves more quickly than that giving rise to the triplet. We reserve the decision of this point until we have been able to apply greater dispersion.

Comparing the sparks obtained with different capacities, it is found that when the spark gap is small, there seems a very curious *diminution* of velocity as the capacity increases; this is not what should have been expected at first sight, as with the large number of jars we should expect higher temperatures, and therefore greater velocity of diffusion. When the spark gap is 1 cm., the experiments do not reveal any marked change due to capacity. When the gap is increased still further the sparks become very irregular and unsteady, and no certain conclusions can be drawn from our measurements; the numbers marked with a query are specially doubtful. When six jars are used practically identical numbers are obtained for all sparking distances, but with small capacity the centimetre spark seems to give a lower result than in the two other cases. While we should not like at present to consider this as an established result, the table serves to show that the centimetre spark and the highest capacity used gives the most consistent numbers, and our experiments with other metals were all made under these conditions, except in the case of bismuth, where clearer spectra were obtained with only two jars.

Comparing different metals with each other, we find in the first place that those having comparatively low atomic weights, viz., aluminium and magnesium, have higher molecular velocities. With magnesium the metal vapour is scattered about to such an extent that no measurements could be made, but the average velocity of the aluminium molecule was found to be over three times as great as that of zinc, the numbers not laying any claim to accuracy. Comparing zinc and cadmium with each other, we obtain almost identical numbers, both for the corresponding doublet and triplets.

Bismuth gave remarkable results. In spite of its high atomic weight some of the lines are but little displaced, indicating an average molecular velocity of 1420 metres/second. For other lines the velocity falls down to that of zinc and cadmium, while one line ($\lambda = 3793$) has a still smaller velocity.

We have not obtained satisfactory results with mercury; the best were those in which poles used were of zinc or cadmium, which were covered with amalgam. Differences in molecular velocities were obtained for different lines, but the result here is not so certain as with bismuth. There is obviously no simple law connecting these velocities with the atomic weight.

Dr. Feddersen was led through his researches to the conclusion that the metallic particles after being once torn off from the electrodes by the discharge took no further part in it, but were thrown irregularly into the space surrounding the electrodes quite independently of the electric current. Although in some cases, and especially with magnesium poles, there is some evidence that this is partly true, we are led to take the following modified view of the matter.

The initial discharge of the jar takes place through the air; it must do so because there is at first no metallic vapour present. The intense heat generated by the electric current volatilises the metal, which then begins to diffuse away from the poles; the subsequent oscillations of the discharge take place through the metallic vapours and not through the air. We find confirmation of this view in a striking experiment which is easily repeated. If a coil of wire be inserted in the spark circuit of a Leyden jar, which may be charged either by a Wimshurst machine or an induction coil, the air lines disappear almost completely, the metallic lines alone remaining. According to our view we should explain the experiment by saying that the coil which adds self-induction lengthens the duration of the discharge, and allows time for the metallic molecules to diffuse properly into the spark gap. A great part of the energy of the current may then do useful work by heating up the metallic molecules instead of those of air. Mr. Hemsalech is at present engaged in investigating the changes in the metallic spectra which accompany the insertion of self-induction.

The first spark passing through the air will give rise to a sound wave which, during the complete time of the discharge, will only travel a few millimetres. We may therefore consider that the mass of metallic vapours suddenly set free is driven by its own pressure into the partial vacuum formed by the heated air. It would seem more correct to liken the process to that of a gas under pressure flowing into a vacuum than to that of a pure thermal diffusion. There is not much difference between these views, and we may take it that in our experiment we have approximately measured the velocity of sound in the metallic vapours. This gives a relation between their temperature and density. If we neglect the differences in the ratio of specific heat we find approximately

$$V = 80 \sqrt{T/\rho},$$

where T is the absolute temperature and ρ the vapour density referred to hydrogen. Thus for cadmium the average molecular velocity found was 560, and substituting $\rho = 56$ we obtain $T = 2700$, which seems a possible value. Hence we conclude that the molecule of cadmium in the spark cannot have a mass which is much smaller than that directly determined near the boiling point of the metal.

In conclusion we have also taken some photographs in which the slit

was directly focussed on the sensitive film without the interposition of the prism. The photographs show a straight image of the slit followed by a number of curved bands extending from both poles into the spark gap.

The straight image we consider to be the initial discharge through air creating sufficient heat to fill the space with vapour through which the oscillating discharges may then pass. Our experiments point to the fact that the periodic time was rather too small in our experiments to give the best results. The metallic molecule before it has had time to reach through a sufficient distance was possibly affected in its motion by the subsequent oscillation. We hope to remedy this defect by introducing still higher capacities than those used. Our experiments allow us to give the following approximate numerical data. The air rendered luminous by the first discharge remains luminous for a time of about 5×10^{-7} second, the metallic vapours then begin to diffuse and reach the centre of the spark (the gap being 1 cm. long) in a time which in the case of cadmium was about 6×10^{-6} second. The periodic time of the oscillations with our six jars and a circuit possessing as little self-induction as possible was about 2×10^{-6} second. The metallic vapours remain luminous in the centre of the spark for a longer period than near the poles, the duration of the time during which some luminosity can be traced with a discharge from six Leyden jars is about $1{\cdot}5 \times 10^{-5}$ second.

February 9, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Reflection of Cathode Rays." By A. A. C. SWINTON. Communicated by LORD KELVIN, F.R.S.

II. "On the Recovery of Iron from Overstrain." By JAMES MUIR, B.Sc. Communicated by Professor EWING, F.R.S.

III. "A Soil Bacillus of the Type of De Bary's *B. megatherium*." By W. C. STURGIS, M.A., Ph.D. Communicated by Professor MARSHALL WARD, F.R.S.

"On the Recovery of Iron from Overstrain." By JAMES MUIR, B.Sc., Trinity College, Cambridge (1851 Exhibition Science Research Scholar, Glasgow University). Communicated by Professor EWING, F.R.S. Received January 25,—Read February 9, 1899.

(Abstract.)

It has long been known that iron which has been overstrained in tension—that is to say, strained beyond the yield-point, so that it suffers a permanent stretch—possesses very different elastic properties from the same iron in its primitive condition. The material is said to be "hardened" by stretching,* since the ultimate effect of such treatment is to raise the elastic limit, and reduce the ductility of the material.

More recently attention has been called to the fact that, primarily, the result of tensile overstrain is to make iron assume a semi-plastic state; so that the elastic limit instead of being raised by stretching is first of all lowered, it may be, to zero.† This plasticity may be shown by applying a comparatively small load to a bar of iron or steel which has just been overstrained by the application and removal of a large stretching load. When the small load is put on, the bar will be found to elongate further than it would had the material been in its primitive state; and a slight continued elongation—a "creeping"—may occur after the small load has been applied. If this load be withdrawn a quite appreciable permanent, or semi-permanent, set will be found to have been produced; a set which diminishes slightly and, if small, may vanish provided time be allowed for backward creeping to take effect. It may also be shown that if the reapplied load be increased the elongation produced will increase in a greater proportion. Thus if a stress-strain curve be obtained from a recently overstrained bar of iron or steel, it will show even for small loads a marked falling away from the straight line which would indicate obedience to Hooke's law.

It is the recovery from this semi-plastic state induced by overstrain to a condition of perfect or nearly perfect elasticity, with raised elastic limit, that is referred to in the title of the paper of which this is an abstract. Such recovery is known to be effected by mere lapse of time,‡ and the object of the experiments described in the paper and

* Ewing, "On certain Effects of Stress," 'Roy. Soc. Proc.,' No. 205, 1880.

† Bauschinger, 'Civilingenieur,' 1881, or 'Mittheilungen aus dem Mech. Tech. Laboratorium in München.' An Account of Bauschinger's work is given in Unwin's book on 'Testing of Materials of Construction.' Ewing, "On Measurements of Small Strains in the Testing of Materials and Structures," 'Roy. Soc. Proc.,' vol. 58, April, 1895.

‡ Bauschinger, 'Dingler's Journal,' vol. 224, p. 5; or 'Mittheilungen aus dem Mech. *Tech. Laboratorium* in München.' Ewing, both papers already cited.

summarised here, is to show the effect of moderate temperature, of mechanical vibration, and of magnetic agitation on this slow return to the elastic state; and further to illustrate this recovery by means of compression tests. One section of the paper deals with the phenomenon of hysteresis in the relation of extension to stress which is exhibited in a marked degree by iron in the overstrained state. Incidentally attention is called to subsidiary points of interest.

The experiments were carried out in the Engineering Laboratory of Cambridge University, and were the outcome of suggestions by Professor Ewing. It was on his suggestion that the effect of moderate temperature on recovery from overstrain was tried, and the result of that trial led to much of the work incorporated in the paper.

The straining and testing were done by means of the laboratory 50-ton testing machine, the specimens employed for the most part being taken from steel rods one inch in diameter, of a quality which may be described as semi-mild. The small strains of extension were measured by Professor Ewing's extensometer.*

After referring to the apparatus and the material employed, and describing the method of experimenting, there are first given in the paper examples of the slow recovery of elasticity with lapse of time. These examples are illustrated by stress-strain curves obtained, at succeeding intervals of time, from extensometer readings similar to those tabulated by Professor Ewing in his paper, referred to above, "On Measurements of Small Strains in the Testing of Materials and Structures." Recovery is shown to be at first comparatively rapid; but latterly very slow progress is made, and weeks or months may be required before an approximately perfect restoration of elasticity is effected. When this is brought about, the specimen may be subjected to a stress a few tons per square inch higher than that at which the virgin material yielded, before a yield-point is passed and the material once more brought into a semi-plastic state. If sufficient time be allowed to elapse after passing this second yield-point, an elastic state will again be assumed, and a third yield-point may be obtained about as far above the second yield-point as the second was above the first. In this manner four or five yield-points may be obtained with the same specimen before fracture occurs. A specimen broken in this manner shows greater ultimate strength, but less ultimate elongation than would have been obtained had fracture been brought about in the usual fashion, that is, without allowing intermediate recoveries of elasticity to take place.

Reference might also be made to Lord Kelvin's discovery of the effect of a Sunday's rest on wires which had been subjected to torsional vibrations throughout the preceding week.

* For description see paper already cited, "On Measurements of Small Strains, &c.," 'Roy. Soc. Proc.,' vol. 58, April, 1895.

The question of recovery of elasticity under stress is next considered in the paper, and it is shown that the process of recovery proceeds at practically the same rate whether the material is kept stressed or is allowed to rest free from load. A slight difference, however, is shown in the two cases, as restoration of elasticity takes place about the position of continued stress.

After this, the phenomenon of hysteresis in the relation of extension to stress is considered, and a closed cycle is shown, having features analogous to those exhibited by a magnetic hysteresis cycle.*

The effect of moderate temperature on recovery from overstrain is next treated of, and it is shown that a slight increase in temperature hastens the restoration of elasticity to a remarkable extent. Three or four minutes at 100° C. proved to be more efficient than a fortnight's rest at the normal atmospheric temperature. The effect of various temperatures below 100° C. is then investigated, and so moderate a temperature as 50° C. is shown to have a large influence in hastening recovery from overstrain. The manner in which recovery proceeds with time when the specimen is kept at a constant temperature is shown in the paper by means of curves. These curves show that at first—that is, before elasticity is fairly well restored—the amount of recovery, measured by the diminution in the elongation produced by a maximum load, is proportional to the square root of the time. For example, the effect produced by, say, four minutes at 80° C. was approximately double of that produced by one minute at the same temperature.

By subjecting an overstrained specimen to temperatures above 100° C., no effect (other than the recovery from the temporary effect of overstrain) was found to be produced until a red heat was almost attained. When the specimen had been subjected to an annealing temperature, of course the whole effect of overstrain was removed, and the material assumed its virgin state.†

After the effect of temperature is discussed, that of mechanical vibration is next recorded in the paper; and it is shown that by striking a recently overstrained specimen with a hammer, so as to make it ring, the material of the specimen is made less elastic. That is, the effect of mechanical vibration is opposite to that of increase of temperature; recovery of elasticity is not hastened, but the material becomes more semi-plastic after mechanical vibration than it was before.

The influence of magnetic agitation is next described. A recently overstrained specimen was subjected to magnetic reversals by means of a coil giving a field strength of 140 C.G.S. units at its centre, but no

* Ewing, "Experimental Researches in Magnetism," 'Phil. Trans.,' 1885, or book on 'Magnetic Induction in Iron and other Metals.'

† See paper by Unwin, "On the Yield-point of Iron and Steel, and the Effect of repeated Straining and Annealing," 'Roy. Soc. Proc.,' vol. 57, 1895.

change whatever was detected in the elastic condition of the material; the process of recovery seemed to be neither accelerated nor retarded.

For the compression experiments described in the paper, an instrument, specially designed by Professor Ewing, was employed to measure the small compressional strains. By the aid of this instrument, the semi-plasticity of recently overstrained iron was readily observed, and the effect of moderate temperature in restoring elasticity was demonstrated by means of compression tests. The lowering of the compression yield-point which accompanies the raising of the tension one (due to tensile overstrain) was also clearly shown. This lowering, however, was not found to be such as to keep the total range of elasticity for the material constant; that is, the lowering of the compression yield-point was not found to be equal to the raising of the tension one.

In conclusion, the characteristics of overstrained iron are considered as illustrating Maxwell's views on the "Constitution of Bodies," as set forth by him in the 'Encyclopædia Britannica.'

"A Soil Bacillus of the Type of De Bary's *B. megatherium.*" By W. C. Sturgis, M.A., Ph.D. Communicated by Professor H. Marshall Ward, F.R.S. Received January 27,—Read February 9, 1899.

(Abstract.)

The organism which forms the subject of these investigations was isolated from clayey and gravelly soil, at a depth of about an inch below the surface. It is a straight, or slightly curved bacillus of rather large size, measuring 3·4—7·7μ × 1·2—1·5μ, and occurs either as isolated rods or in the form of long chains. Its peculiar interest lies in the fact of its marked predilection for acid media, and its behaviour in the presence of carbohydrates. It also offers peculiar advantages for the study, by direct observation in hanging drops, of the formation and germination of the spores, the formation of gelatinous sheaths, the co-existence of motile and non-motile stages, and the rejuvenescence of so-called involution forms, the former process especially being very rapid under suitable conditions.

The organism forms on solid, acid media, such as saccharose-gelatine, large, domed, translucent drops, consisting of chains of rods provided with thick, firm, and gelatinous sheaths. The latter character is exhibited only in media containing carbohydrates, especially cane sugar; in media devoid of carbohydrates the colonies are slimy rather than gelatinous, and consist of almost naked rods and chains. Moreover, even in the presence of sugar, the formation of an investing sheath is *largely* dependent upon the age of the culture, the vigour of the

material, and the degree of condensation of the medium. Thus if a gelatine culture becomes liquefied, either by the normal liquefying action of the bacillus itself, or by the addition of water, the sheath gradually dissolves, and the colonies disintegrate into flocculent masses of rods possessing only very thin sheaths. In liquid cultures the rods are practically non-capsuled from the beginning, and the same is true of cultures on solid, saccharine media, if the material used has been previously attenuated by being subjected to temperatures below 15° C. As has been said, the organism slowly liquefies sugar-gelatine; on peptone-gelatine the same effect is produced, but more rapidly. In neutral media growth is extremely slow, even under the most favourable conditions otherwise.

The growing organism is somewhat intolerant of low temperatures, and very susceptible to changes of temperature, a variation of two or three degrees exercising a very marked effect upon the rate of growth. It is probable that the minimum temperature for growth is in the neighbourhood of 10° C., and the maximum about 35° C. At a temperature of 22° C., growth is rather slow as compared with some other species, notably *B. subtilis*, Ehr. The spores are able to withstand a temperature of 100° C. maintained for five minutes. Although the organism is normally aërobic, growth occurs with almost equal vigour *in vacuo* or in pure hydrogen as in air, provided that vigorous material is used, and that both the medium and the temperature are favourable. It is therefore a facultative anaërobe.

When growing directly on the surface of the gelatine, the colonies tend to take the form of coiled and twisted strands, composed of long, parallel, septate filaments.

In milk, the organism produces peptonisation and an alkaline reaction, but without coagulation. On potato the growth is spreading, whitish and slimy or viscous, and consists of non-capsuled rods. Later it becomes dry and cheesy, and eventually distinctly yellow in colour.

On agar, whether in the presence of sugar or peptone, flat, circular, cheesy colonies are produced, which are tawny in colour, and emit a strong odour resembling melted glue.

Spores are produced abundantly on various media, the time required varying from twenty-two to seventy hours, according to the medium and the temperature. Spore-formation proper is preceded by peripheral condensation of the cytoplasm on the walls of the rod, leaving one or two large central vacuoles. The bulk of the cytoplasm then collects at one end of the rod, minute portions being left behind as a thin, irregular lining on the walls, and on the septum dividing the rod from its neighbour. The distal portion of the cytoplasm then condenses, and finally forms an oval spore, occupying an oblique position in one end of the rod, and measuring $2—2{\cdot}8\mu \times 0{\cdot}8—1\mu$. It escapes by the dissolution of the thinner portion of the wall. The spore

germinates at one end in a direction parallel to the longer axis. The subsequent growth of the chains is intercalary as well as terminal. The rate of growth was obtained by taking consecutive measurements of vigorous rods or chains, and noting the time required for the original length to double. This time, according to the terminology suggested by Marshall Ward, is known as the "doubling period." In this case the doubling period in saccharose-broth at 22·5 °C. is forty-eight minutes, at 19·5—20·5° C., 120 minutes. The slow growth of this organism at these temperatures, and its sensitiveness to slight changes of temperature, are well illustrated by these and similar observations.

When united in long chains the organism is non-motile, but rods and short chains freshly produced from spores or involution-forms in liquid media exhibit active motility, especially at temperatures between 23° C. and 33° C. This consists of progressive, undulating, and rotary motions, and may last for several hours. It is confined to isolated rods, and to chains of not more than three to six individuals. Progressive motion is never observable in chains of more than three united rods.

Involution-forms are produced commonly in old cultures, or in cultures made at low temperatures. They consist of enormously swollen, fusiform, or drum-stick bodies, the contents of which are composed of coarsely granular cytoplasm and numbers of oily globules. In some cases the cytoplasm is found to have undergone plasmolysis, but if this has not gone too far, the involution forms are able to develop into normal rods if transferred to a suitable environment. The organism is non-pathogenic, produces no pigment or evolution of gas, and stains readily with carbol-fuchsine, aniline-gentian-violet, or by Gram's method.

In conclusion it may be said that this bacillus presents many points of resemblance to certain well-known species, but at present it may be impossible to refer it accurately. From *B. aërophilus*, Lib., it is distinguished by its facultative anaërobism, its colour, its mode of liquefaction, &c.; from *B. subtilis*, Ehr., *B. vulgatus* (Flügge), Mig., and *B. mesentericus* (Flügge), Lehm. and Neum., by various minor characters. The description of Russell's *B. granulosus* is not sufficiently detailed to make an accurate comparison possible. That it is closely allied to De Bary's *B. megatherium* is very evident, and it is quite possible that prolonged investigation of the two forms side by side may prove them to be identical. In that case added interest will attach to my investigations, in the way of showing the remarkable variations which may be produced within the limits of a single species, by different methods of treatment, De Bary's form being nearly twice as thick as this one.

February 16, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced the acceptance by the Council of a portrait of Lord Kelvin, presented to the Society by Dr. Thorpe, on behalf of a large number of the Fellows.

The following Papers were read:—

I. "On the Reflex Electrical Effects in Mixed Nerve, and in the Anterior and Posterior Roots." By Miss S. C. M. SOWTON. Communicated by Dr. A. D. WALLER, F.R.S.

II. "The Characteristic of Nerve." By A. D. WALLER, M.D., F.R.S.

III. "Observations on the Cerebro-spinal Fluid in the Human Subject." By STCLAIR THOMSON, M.D., L. HILL, M.B., and W. D. HALLIBURTON, M.D., F.R.S.

IV. "The Thermal Deformation of the Crystallised Normal Sulphates of Potassium, Rubidium, and Cæsium." By A. E. TUTTON, B.Sc. Communicated by Captain ABNEY, F.R.S.

"Observations on the Cerebro-Spinal Fluid in the Human Subject."

By STCLAIR THOMSON, M.D., LEONARD HILL, M.B., and W. D. HALLIBURTON, M.D., F.R.S. Received January 31,—Read February 16, 1899.

One of us (StC. T.) has had under his care for some years a young woman who has suffered from continuous dripping from the nose. The case has not been amenable to any treatment. At first it was thought to be one of nasal hydrorrhœa, but certain characters in the affection convinced the observer that this could not be so, and that the fluid, which dropped from one nostril only, was cerebro-spinal fluid. This was supported by the results of the chemical examination of the fluid. The escape of cerebro-spinal fluid from the nose has long been known to follow traumatic injury to the cribriform plate of the ethmoid bone, but the possibility of its spontaneous escape from the nose does not

appear to have been fully established before the present instance. However, considerable research into the literature of the subject has shown that there are several cases recorded in which, though no history of injury existed, the flow of fluid from the nose was of such a character that they must have been similar to the present case, although in the majority of instances the true nature of the fluid escaped observation.

Many of these patients exhibited cerebral symptoms in the course of the disease, and some ultimately died from inflammation of the cerebral meninges, which had probably spread from the nose through some opening in the bony lamina that normally separates the cranial and nasal cavities. The full clinical details of this rare case and of the similar ones just referred to are, however, reserved for publication elsewhere. The present paper is concerned only with the composition of the fluid and the variations it presents under different circumstances.

Characters of the Fluid.

Our opportunities for examining the fluid chemically have been fairly frequent. The fluid was always collected in sterilised glass vessels, and the examination made as soon as possible by one of us (W. D. H.) at King's College, London.

The fluid is perfectly clear and colourless, looking like water; its reaction is faintly alkaline; its specific gravity is about 1005. On microscopic examination it shows no cells or other deposit. It gives no precipitate with acetic acid. It contains a trace of proteid, coagulable by heat, but the quantity is too small to give more than an opalescence. In another portion of the fluid it was ascertained that this proteid is precipitable by saturation with magnesium sulphate; it is therefore a globulin. Albumin and other proteids are absent.

The fluid contains a substance which reduces Fehling's solution. A portion of the fluid was treated with excess of acidified alcohol; the proteid so precipitated was filtered off. The filtrate was evaporated to dryness over a water bath; the dry residue was taken up with alcohol, filtered, and again evaporated to dryness. Part was evaporated to dryness on a glass slide; the residue examined microscopically was seen to contain the needle-like crystals, occurring singly and in bundles, similar to those previously described and figured by one of us (W. D. H)* as obtainable from cerebro-spinal fluid. The residue had also the characteristic pungent taste of pyrocatechin.

The remainder of the dry residue was dissolved in water and filtered. The filtrate reduces Fehling's solution well, but it does not ferment with yeast, nor does it give any osazone crystals on treatment with phenylhydrazine hydrochloride and sodium acetate. Control experiments with a weak solution of dextrose, which gave about the same

* 'Journ. of Physiol.,' vol. 10, p. 248.

amount of reduction with Fehling's solution, gave both these tests in a typical way.

The fluid was tested for creatinine with negative results.

The same results relative to the reducing substance have been obtained over and over again in various specimens of this fluid. They agree with the observations of nearly all writers on the cerebro-spinal fluid, but differ from those of Nawratski,* who in a recent paper has affirmed, principally from observations on the cerebro-spinal fluid of the calf, that the reducing substance present is dextrose.

The principal points to be noticed in the properties of the fluid which lead to the conclusion that it is cerebro-spinal fluid are the following:—

(1) Its clear, watery character.
(2) Its low specific gravity.
(3) The small amount of proteid in it and the absence of albumin.
(4) The presence in it of a substance which reduces Fehling's solution, but is not dextrose. It is possibly a substance related to pyrocatechin.

In comparison with this fluid, we examined also the secretion in some cases of true nasal hydrorrhœa. This fluid is opalescent, somewhat viscid, and on microscopic examination shows amorphous matter with mucous corpuscles. It gives with acetic acid a precipitate of mucinoid nature. It sometimes does and sometimes does not contain a reducing substance, and this substance when present is sugar.

A quantitative analysis of one of these nasal fluids showed that the percentage of solids, especially organic solids, is higher than in cerebro-spinal fluid. The results of the analysis are as follows:—

	Per cent.
Water	98·792
Total solids	1·208
Proteids (including mucin)	0·260
Other organic substances	0·163
Inorganic substances	0·785

Our observations on the characters of the cerebro-spinal fluid were followed by others in which we sought to answer the following questions:—

The rate of flow.
The difference of composition at different times of the day.
The influence of straining, posture, and abdominal compression on the flow and composition of the fluid.
The effect on blood pressure of intra-venous injection of the fluid in animals.

* 'Zeits. f. Physiol. Chem.,' 1897, vol. 23, p. 532.

The Rate of Flow.

One portion, collected by the patient herself in the course of an hour, measured 4 c.c. Another portion, collected under the supervision of one of us (StC. T.) in ten minutes, measured 3·9 c.c.

If the first portion is taken as a measure of the rate of secretion, the amount formed in the day will be 96 c.c. Taking, however, the second observation as being more accurate, the amount formed in the twenty-four hours will be over half a litre (561·6 c.c.). It is possible that this estimate is too high, as doubtless the patient, being under the observation of a physician, would be somewhat excited, and the consequent alteration of the circulation would, as we shall immediately see, cause the flow to become more abundant.

Comparison of the Morning and Evening Fluid.

Cavazzani,* from experiments on dogs, found that the cerebro-spinal fluid collected in the morning was more alkaline than in the evening, and contained more solid residue. He considers that this is related to the activity of the nervous system, and that it confirms Obersteiner's theory of sleep. He obtained corresponding results in the case of a man with traumatic fistula of the frontal bone.

We considered it worth while to repeat this observation.

The qualitative examination of the fluid collected first thing on several mornings gave the same results as that of specimens collected the last thing in the evening. Both were distinctly alkaline, but no estimation of the relative alkalinity was made. The following table gives in percentages the results of the quantitative analyses:—

	Morning fluid.	Evening fluid.
Water	99·004	99·027
Solids	0·996	0·973
Organic solids	0·118	0·100
Inorganic solids	0·878	0·873

The evening fluid is thus slightly poorer in both classes of constituents than that of the morning; the difference is chiefly due to an alteration in the organic solids. This is just what we should expect, as the decreased capillary pressure during sleep would lessen the rate of exudation of water. Without committing ourselves to any theory on nervous activity or sleep, we may say that our experiments confirm those of Cavazzani.

* "Sul Liquido Cerebro-spinale," 'La Riforma Medica,' Anno VIII, 1892, *vol. 2, p. 591.*

The Influence of Straining and Posture on the Flow and Composition of the Fluid.

In a monograph on the cerebral circulation* one of us (L. H.) put forward the view that the rate of secretion of the cerebro-spinal fluid, when the cranio-vertebral cavity is opened, depends directly on the difference between the pressure in the cerebral capillaries and that of the atmosphere. At the same time it was shown that cerebral capillary pressure varies directly and absolutely with vena cava pressure. Thus the cerebral capillary pressure can be raised with great ease by any agency which causes a rise of pressure in the vena cava or cerebral veins. On the other hand, cerebral capillary pressure varies directly, but only proportionately, with aortic pressure, for between the aorta and the capillaries there lies the peripheral resistance.

It follows from the above that the easiest methods of raising the cerebral capillary pressure in man are:—

(*a*) By compression of the abdomen.
(*b*) By the assumption of the horizontal posture. In this position, however, the rise of venous pressure may be compensated by the fall of arterial pressure, which normally occurs when the body is at rest. This is, no doubt, the case during sleep.
(*c*) By straining or forced expiratory effort, with the glottis closed.

By all these methods the vena cava pressure is considerably raised; and by the last method the venous inlets into the thorax may be completely blocked, and the pressure in the cerebral capillaries raised to something like aortic pressure.

It is true that by such a forced expiratory effort the aortic pressure is lowered. Nevertheless, the total effect on capillary pressure is a very great rise, for a fall of aortic pressure of 25 mm. of mercury produces a fall in cerebral capillary pressure of less than 5 mm. of mercury, while a rise of vena cava pressure of 25 mm. of mercury produces a rise of cerebral capillary pressure of 25 mm. Hg.

The present case gave us a unique opportunity of testing the correctness of these views on the living human subject, and our experiments entirely confirm them. As will be seen from the following figures, the flow of cerebro-spinal fluid is accelerated by all those circumstances which raise the cerebral capillary pressure. The increase in flow is, moreover, accompanied by a decrease in the percentage of solid matter.

The experiments were conducted under the supervision of two of us (StC. T. and L. H.); the chemical investigation of the fluid was performed, as before, by the third (W. D. H.).

* 'The Physiology and Pathology of the Cerebral Circulation,' by Leonard Hill, London, Messrs. Churchill, 1896.

1. Patient sitting quietly without straining. In five minutes 23 minims (1·357 c.c.) were collected.

2. Patient sitting and straining. In five minutes 35 minims (1·965 c.c.) were collected.

3. Patient sitting quietly. In five successive minutes the amounts collected were, respectively, 8, 7, 5, 5, 5 drops. The total measured 19 minims (1·021 c.c.).

4. Subsequent to this, five minutes were occupied by the patient in straining, and the amounts collected in consecutive minutes were 12, 10, 8, 9, and 10 drops respectively. The total measured 33 minims (1·947 c.c.).

5. Patient lying down and not straining. The drops fell as follows in five consecutive minutes—9, 6, 5, 5, and 5, and the total measured 27 minims (1·593 c.c.). Here the arterial pressure was probably not decreased owing to mental excitement, while the cerebral venous pressure was increased.

6. Patient lying flat on the stomach and head hanging over the end of a sofa. The drops fell as follows in five consecutive minutes, 8, 7, 6, 7, and 7. The total measured 28 minims (1·652 c.c.).

7. Finally, after the last experiment, the following was collected during quiet dropping, while the patient was sitting with the head forward. The drops fell as follows:—5, 4, 4, 4, and 4, in five successive minutes; and the total measured 15 minims (0·885 c.c.).

The following is the report on the chemical examination of the fluids:—

So far as the small quantities available admit of analysis, the fluids are the same qualitatively. The liquid which escaped passively, and that which passed under straining, both contained a small quantity of organic and inorganic solids. Among the organic substances present are the reducing substance and a trace of proteid. Judged by the amount of precipitate produced by alcohol in equal amounts of the two fluids, the proteid is less abundant in the fluid passed during straining, but the amount is too small to weigh.

Determination of the total solids gave the following results, expressed in percentages:—

A. The fluid passed passively, 1·1 per cent.

B. The fluid passed during straining, 0·43 per cent.

Even the higher of these numbers is less than in cases of cerebro-spinal fluid from meningocele and hydrocephalus, previously recorded by one of us (W. D. H.).*

In addition to the foregoing, two specimens were collected at home by the patient herself. Analysis of these gave the following results:—

A. Fluid collected while patient was sitting upright quietly. The percentage of solids was 1·11.

* 'Journ. of Physiol.,' vol. 10, p. 232.

B. Fluid collected while she was lying down. The percentage of solids was 1·03.

The effect of the horizontal posture is in the same direction, though not so marked as the effect of straining. This is what was to be expected, for the horizontal posture would not raise the venous, and thus the cerebral, capillary pressure so much as powerful expiratory efforts would. Moreover, the arterial pressure falls during quiet rest in the recumbent posture, as one of us has determined (L. H.).*

In order to note the effects of straining on the retinal circulation, Mr. Vernon Cargill was asked to examine the patient, and he kindly reported as follows:—"I noticed that when a straining effort was made, a decided but transitory narrowing of the retinal arteries on and adjacent to the disc, and also a marked pulsation in the trunks of the retinal veins occurred."

The transitory narrowing of the arteries points to the temporary lowering of the aortic pressure, while the pulsation of the veins is a sign of the capillary engorgement due to venous congestion.

Experiments made with Abdominal Compression.

These experiments were made in order to complete and confirm those just recorded. The patient was seated, and the abdomen was compressed as firmly and evenly as possible by one of us (StC. T.), spreading both hands over the front of the abdomen. The number of drops per minute were counted as before, and periods of compression lasting five minutes were alternated with periods of the same duration, during which the patient was sitting quietly.

The following table gives the results succinctly:—

Condition of patient.	Drops in successive minutes.	Total collected.	
		Minims.	c.c.
A. Abdomen compressed......	11, 9, 8, 7, 5	27	1·593
B. Sitting quietly............	4, 5, 3, 4, 4	14	0·826
C. Abdomen compressed......	11, 8, 8, 6, 6	24	1·416
D. Sitting quietly............	6, 7, 8, 6, 6	Measurement omitted	

The fluids from experiments "A" and "C" were mixed together; also those from experiments "B" and "D." Determination of the total solids gave the following results:—

"A" and "C." Fluid collected during abdominal compression. Percentage of solids, 0·68.

"B" and "D." Fluid collected while the patient was sitting upright quietly. Percentage of solids, 1·14.

* 'Phys. Soc. Proc.,' January 15, 1898.

The experiments confirm those recorded in the preceding section. Abdominal compression raises the vena cava pressure, and so leads to increased cerebral capillary pressure, and in this way to increase in the volume of the cerebro-spinal fluid secreted. Increase of volume, as before, is accompanied with fall in the percentage of solids present.

Intra-vascular Injection of the Cerebro-spinal Fluid.

One of us (W. D. H.), in conjunction with Dr. Mott, F.R.S., has been for some time engaged in examining the results of injecting into animals cerebro-spinal fluid removed from cases of brain atrophy, especially from cases of general paralysis of the insane. This fluid contains a toxic substance, choline, doubtless derived from the disintegration of lecithin in the brain. Injection of such fluid into the jugular vein of animals (dogs, cats, rabbits), anæsthetised with ether, causes a marked lowering of arterial blood pressure, which is partly cardiac in origin, but principally due to the local action of the poison on the neuro-muscular apparatus of the peripheral vessels, especially in the splanchnic area.*

The fluid obtained from the present case was also injected in a similar way. Quantities varying from 7 to 10 c.c. were injected into the circulation in dogs, but with entirely negative results. Such a quantity in the case of fluid from a general paralytic would be quite sufficient to cause a marked fall of arterial pressure.

Similar negative results, both as regards blood pressure and respiration, were obtained with other specimens of normal cerebro-spinal fluid removed from other animals, or from cases of meningocele and hydrocephalus in children. In all such cases, also, choline was searched for chemically, but with negative results.

"The Thermal Deformation of the Crystallised Normal Sulphates of Potassium, Rubidium, and Cæsium." By A. E. Tutton, B.Sc. Communicated by Captain Abney, C.B., F.R.S. Received January 31,—Read February 16, 1899.

(Abstract.)

In this memoir are communicated the results of sixty-four determinations of the thermal expansion of the orthorhombic crystals of the normal sulphates of potassium, rubidium, and cæsium, carried out for the three axial directions of the crystals with the aid of the compensated interference dilatometer previously described by the author.†

* 'Physiol. Soc. Proc.,' Feb., 1897, and Feb., 1898 ('Journ. of Physiol.,' vols. 21 and 22).

† '*Phil. Trans.*,' A, vol. 191, p. 313.

The employment of the compensated method has proved highly successful, extremely concordant results being afforded with crystals not necessarily more than 5 mm. thick. The twenty-nine different parallel-faced crystal-blocks employed varied in thickness from 4·8 to 10·7 mm. The main conclusions from the work are given in the following summary.

The coefficients of cubical expansion exhibit a progression, corresponding to the progression of the atomic weights of the three respective metals. This is true of both the constants a and b in the general expression for the coefficient of cubical expansion, $a = a + 2bt$, for any temperature t. The actual values are shown in the following table:—

	a.	b.
K_2SO_4	0·000 104 75	0·000 000 069 8
Rb_2SO_4	0·000 103 14	0·000 000 076 7
Cs_2SO_4.........	0·000 101 70	0·000 000 081 0

The order of progression of the two constants is inverted; a, the coefficient for 0°, diminishes with increasing atomic weight of the metal, while b, half the increment of the coefficient *per* degree of temperature, increases. Consequently, the coefficients of cubical expansion of the three salts converge, with rise of temperature, and attain equality in pairs. Identity is attained for potassium and rubidium sulphates at 114°, for potassium and cæsium sulphates at 136°, and for rubidium and cæsium sulphates at 168°. At 136° equality for all three is only deviated from by one unit in five hundred. Beyond the temperature of identity divergence occurs, and an increase of atomic weight is now accompanied by an increase of expansion.

The thermal deformation is of the nature of an expansion in all directions in the crystals of all three sulphates.

The differences between the coefficients of linear expansion along the three axial directions of any one salt, although only amounting to one-eighth of the total coefficient, are large compared with the differences between the values for the same direction of the three salts. This, together with the fact that the replacement of one metal by another is accompanied by considerable modifications of the relations of two of the three values for the original salt, those corresponding to the axes a and c, prevent the coefficients of linear expansion for any one direction of the three salts from exhibiting any progression corresponding to that of the atomic weights of the three metals. These directional perturbations are, however, mutually compensative, so that the effect of interchange of the metals is clearly exhibited by the solid deformation, the cubical expansion, in the progressive manner already indicated.

The increment of the linear coefficient of expansion along the axis c of each salt is about twice as large as the increments for the other two

directions, a and b, for which latter the increments are nearly equal. This is analogous to the optical behaviour, the refractive power being altered (diminished) by rise of temperature much more in the direction of the axis c than in the other two directions, in which the lesser amounts of change are nearly equal.

The amount of expansion along the direction of the axis b is approximately identical for all three sulphates, indicating that interchange of the metals is without influence on the thermal behaviour along this axis. The crystals of all three salts expand least in this direction, which is, therefore, the common minimum axis of the thermal ellipsoid.

The chief of the directional perturbations previously referred to consists of a reversal, for temperatures below 50°, of the directions of the maximum and intermediate axes of the thermal ellipsoid for rubidium sulphate, compared with their directions in the potassium and cæsium salts. The maximum thermal axis is c for the two latter salts, but a for rubidium sulphate. A similar reversal of the direction of the maximum axis of the optical ellipsoid (the indicatrix), the first median line, from c to a, occurs for the same temperatures, in the case of rubidium sulphate. The maximum thermal axis is identical with the first median line in all three salts.

At high temperatures the same relations continue to hold for the potassium and cæsium salts, both thermally and optically. But owing to the increment of expansion along c being so much greater than for the other directions, the intermediate expansion along c for rubidium sulphate attains equality at 50° with the expansion along a, and beyond this temperature c becomes the maximum thermal axis for this salt, as it is for the other two sulphates. Consequently, at 50° the crystals of rubidium sulphate are apparently thermally uniaxial. At temperatures varying 10° each side of 50° for different wave-lengths of light, they have previously been shown to simulate uniaxial optical properties. The thermal and optical ellipsoids of revolution are not, however, identically orientated, the axis of the former being b and of the latter a. Further, the change of direction of the maximum thermal axis of rubidium sulphate from a to c is followed optically at 180° by the change of the first median line from a to c, rendering the rule as to coincidence of the maximum thermal axis and the first median line again valid.

A close parallelism between the linear thermal expansion and the directional optical behaviour is thus found to exist. The optical constants have been shown in a previous memoir* to exhibit a clear progression following the order of progression of the atomic weights of the three alkali metals; the values for the three salts are very much more widely separated than in the case of the linear thermal constants,

* 'Journ. Chem. Soc., Trans.,' 1894, p. 628.

and consequently the progression is undisturbed by the modification of the directional differences for the same salt, which are relatively so much more important in the case of the thermal constants.

The net effect of the replacement of one metal by another has, however, been shown to be clearly demonstrated by the progression of the coefficients of the cubical expansion and their increments.

The final conclusion of the investigation, therefore, is that:

The thermal deformation constants of the crystals of the normal sulphates of potassium, rubidium, and cæsium exhibit variations which, in common with the morphological, optical, and other physical properties previously investigated, follow the order of progression of the atomic weights of the alkali metals which the salts contain.

"On the Reflex Electrical Effects in Mixed Nerve and in the Anterior and Posterior Roots." By Miss S. C. M. SOWTON. Communicated by A. D. WALLER, M.D., F.R.S. Received December 12, 1898,—Read February 16, 1899.

The following experiments were made during the months of May, June, and July, 1897, in the Physiological Laboratory of Leipzig, under the guidance of Professors Hering and v. Frey, to test whether in the frog, reflex electrical changes could be demonstrated at the central end—

I. Of a mixed nerve.
II. Of anterior roots alone.
III. Of posterior roots alone.

As regards the first two heads, the end in view was simply the actual verification of an extremely probable phenomenon, preparatory to an examination of the third head, viz., reflex electrical effects propagated down the posterior roots, which, in 1891,* were pointed out by Gotch and Horsley, and offered as proof of the passage of centrifugal nerve impulses in normally afferent nerve channels. The results obtained in the present experiments being somewhat difficult to interpret, the notes were laid aside until opportunity should offer for carrying the investigation further. Professor Bernstein having, however, quite recently† discussed the question of the reflex negative variation of the nerve current, the moment seemed opportune for submitting these results as they stand to the attention of those interested in the subject.

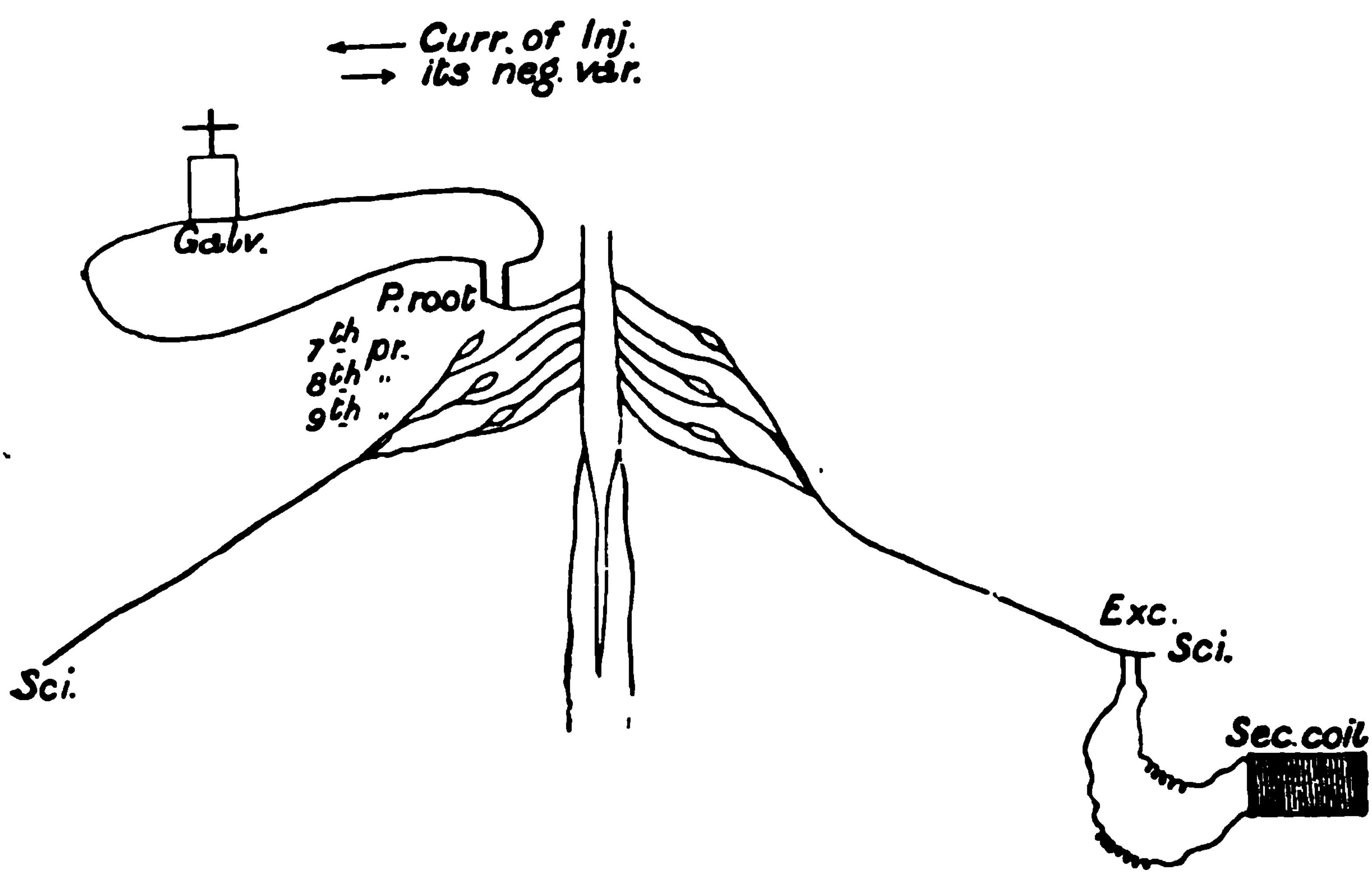
The galvanometer used was on the lines of Thomson's reflecting instrument, with modifications by Carpentier. The leading-off electrodes had finely pointed camel's hair brushes inserted in the plug

* 'Phil. Trans.,' B, vol. 182.

† 'Pflüger's Archiv,' vol. 73, p. 374, 1898.

of refined clay, the brushes being moistened with normal saline. The current of injury was compensated in the usual way. In the exciting circuit, the Du Bois Reymond induction coil was supplied by a single Daniell cell, the stimulating electrodes were of platinum wire, and had an extra loop of wire (Hering's pattern) to guard against unipolar effects.

Frogs of the Esculenta species were used; they were cooled, *i.e.*, kept for six or seven days on ice to ensure a high degree of excitability. The frog was prepared with as little loss of blood as possible, the brain above the medulla being destroyed by insertion of a small peg of wood. For experiments under head I, the frog having been firmly secured, the two sciatics were exposed, cut at the knee-joint, and isolated in their full extent. The nerve of one side was then raised, supported by a glass hook, and connected at transverse and longitudinal surfaces with the galvanometer electrodes—the hook was so arranged as to obviate any possible shifting of contacts. The sciatic of the opposite side was then also raised, and its lower end laid across the stimulating electrodes. In one or two preliminary experiments, one branch only of the sciatic was divided at the knee and led off to the galvanometer, the other branch being left in connection with its muscles to serve as a control; under these conditions, on stimulating the sciatic of the opposite side, the electrical and muscular effects corresponded, both sometimes failing to appear.



I.—Experiments showing Reflex Electrical Changes at the Central End of a Mixed Nerve.

	Part stimulated.	Part led off.	Deflection of galvanometer.
May 8, 1897. Coil at 30 cm.	Central end of right sciatic.	Central end of opposite sciatic.	5° and rather more.
May 17. Frog curarised. Coil 30 cm.	Do. (Effect the same sciatic.)	Do. from left to right	10—30°.
May 20. Frog curarised. Coil 30 cm.	Do.	Do.	Trace.
Coil at 30 cm. Effect much the same with coil at 12.	Do.	Do.	40—50°.
May 22	Do.	Do.	10—15°.
	Strychnia injected	subcutaneously.	
	Do.	Do.	40—90°. Also clonus effects 120—130° at highest, in response to a light tap on the table.
May 24	Central end of right sciatic.	Central end of opposite sciatic.	10—20°.
	After	strychnia.	
	Do.	Do.	Slight increase up to 30°.
2nd Frog	Do.	Do.	10°.
	After	strychnia.	
	Do.	Do.	30—70°.
May 29. Frog etherised.	Do.	Do.	No deflection either way across.
	After	strychnia.	
	Do.	Do.	20—35°.
June 18. Coil 15 cm.	Do.	Do.	10—15°.
June 23. Coil 10 cm.	Do.	Do.	4—10°.
2nd Frog	Do.	Do.	5°.

In these experiments the negative variation was reflex in character, effects could not be obtained in rapid succession, but a pause of a

minute or two was necessary before repeating the stimulation, and the latent period was very marked—to be reckoned often in seconds.

In experiments where the nerve roots were to be led off to the galvanometer, the frog was always curarised. A sciatic having then been prepared as before for stimulation, its nerve roots (7th, 8th, and 9th pairs), or the corresponding pairs of the opposite side, were exposed by opening up the lower part of the spinal column; the roots were then carefully separated and those to be led off to the galvanometer were cut as far as possible from the cord (just above the ganglion, in the case of posterior roots). Two roots were usually taken together and their central ends connected with the brush electrodes.

In cases where the bulb was stimulated, the brain and upper half of the cord were exposed, and the brain cut off at the bulb. The cord having then been severed from its attachments, was carefully raised and the bulb laid upon the stimulating electrodes.

II.—(1) Experiments showing Electrical Changes at the Central End of Anterior Roots alone. Excitation of Sciatic of same side.

	Part stimulated.	Part led off.	Deflection of galvanometer.
May 12. Coil 30 cm.	Central end of left sciatic.	Central end of 2 anterior roots of same side.	10—40°.
June 21. Coil 15 and 10 cm.	Do.	Central end of 1 anterior root of same side.	5—10°.

II.—(2) Ditto, ditto. Excitation of Bulb.

	Part stimulated.	Part led off.	Deflection of galvanometer.
June 25. Coil 10 cm.	Bulb.	Central end of 2 anterior roots.	20—100°.
June 29. Coil 10 cm.	Do.	Do.	5—10°.

III.—(1) Experiments showing Electrical Changes at the Central End of Posterior Roots alone. Excitation of Sciatic of same side.

	Part stimulated.	Part led off.	Deflection of galvanometer.
June 21. Coil 30 cm.	Central end of left sciatic.	Central end of 2 posterior roots of same side.	5—8°.
June 22. Coil 10 cm.	Do.	Do.	4—10°.
July 10. Coil 10 cm.	Do.	Do.	5—6°.
July 12. Coil 10 cm.	Do.	Do.	10—20°.
July 15. Coil 10 cm.	Do.	Do.	2°
July 16. Coil 10 cm.	Do.	Do.	3°

III.—(2) Ditto, ditto. Excitation of Sciatic of opposite side.

	Part stimulated.	Part led off.	Deflection of galvanometer.
June 21. Coil 10 cm.	Central end of right sciatic.	Central end of 2 opposite posterior roots.	2—3°
July 10. Coil 10 cm.	Do.	Do.	8—9°
July 12. Coil 10 cm.	Do.	Do.	8—15°

In these last nine experiments where the lead-off was from posterior roots the deflection had not the characters of a reflex deflection—viz., delay and exhaustibility; the negative variation followed at once on stimulation and could be repeated without the pause necessary in the case of an ordinary reflex. The same thing was also observed on one occasion in the case of anterior roots (experiment of June 29). In the remaining experiments III (3) where, the bulb being stimulated, the posterior roots were led off to the galvanometer, this non-reflex effect was observed in certainly two cases (June 29 and 30).

III.—(3) Ditto, ditto. Excitation of Bulb.

	Part stimulated.	Part led off.	Deflection of galvanometer.
June 25. Coil 10 cm.	Bulb.	Central end of 2 posterior roots.	10—15°
June 29. Coil 10 cm.	Do.	Do.	3—7°
2nd Frog.	Do.	Do.	3—9°
June 30. Coil 10 and 15 cm.	Do.	Do.	10—17°
2nd Frog...... ...	Do.	Do.	3—4°
July 1. Coil 10 cm.	Do.	Do.	2—3°
2nd Frog	Do.	Do.	5—10°
July 2. Coil 10 cm.	Do.	Do.	2—3°
July 5. Coil 10 cm.	Do.	Do.	5—9°

No exact quantitative estimation of electrical effects was aimed at in these experiments, but as a rough term of comparison, the negative variation of a directly stimulated sciatic was occasionally noted, this deflection was seldom less than 250° of the scale. The average effects obtained in the six series of experiments are given below.

	Part stimulated.	Part led off.	Deflection of galvanometer.
	Ordinary negative variation from sciatic		250°
I. (1)	Central end of sciatic (9 expts.).	Central end of off sciatic.	14°
II. (2)	Central end of left sciatic (2 expts.).	Central end of same anterior roots.	16·2°
II. (3)	Bulb (2 expts.).	Central end of anterior roots.	33·5°
III. (4)	Central end of left sciatic (6 expts.).	Central end of same posterior roots.	6·5°
III. (5)	Central end of right sciatic.	Central end of opposite posterior roots.	7·5°
III. (6)	Bulb (9 expts.).	Central end of posterior roots.	6·6°

Throughout the experiments fallacies arising through induction were carefully guarded against, the negative variation was tested by reversing the direction of the stimulating current; and as a final test for current escape the led-off nerve was cut through with wet scissors in such a way that the severed ends remained in contact, though physiological continuity was destroyed; the stimulation was then repeated, but in no case was there any deflection after such section.

In the main my results exhibit—

I. Electrical effects of an indubitably reflex character in the mixed nerve and in the anterior roots alone.

II. Slight effects of doubtful character in the posterior roots.

My warmest thanks are due to Professor Hering for the courteous hospitality of his laboratory, and to Professor von Frey I would offer my grateful acknowledgment of his ready help.

February 23, 1899.

The LORD LISTER, F.R.C.S., D.C.L., in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Order of Appearance of Chemical Substances at different Stellar Temperatures." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

II. "The Efficiency of Man, or Economic Coefficient of the Human Machine." By Dr. W. MARCET, F.R.S., and R. B. FLORIS.

III. "Some Experiments bearing on the Theory of Voltaic Action." By J. BROWN. Communicated by Professor EVERETT, F.R.S.

IV. "Deposition of Barium Sulphate as a Cementing Material of Sandstone." By Dr. F. CLOWES. Communicated by Professor ARMSTRONG, F.R.S.

"The Efficiency of Man, or Economic Coefficient of the Human Machine." By W. Marcet, M.D., F.R.S., and R. B. Floris F.C.S.

Received February 3,—Read February 23, 1899.

(From the Physiological Laboratory, University College, London.)

In a paper communicated by one of us, in April last, to the Royal Society,* a calorimeter was described for determining the heat emitted by man, which, on being tested, was found to give very reliable results. At the same time a short history was added of the various instruments used by physiologists to estimate the heat given out by man and animals; from that list was accidentally omitted the calorimeter of D'Arsonval.†

This calorimeter consists of two cylindrical concentric vessels, with an air space between them, and standing in an annular groove full of water, thus making an air-tight joint. The outer chamber (annular space) has a manometer connected with it which registers in a graphic way the variations of pressure in that chamber throughout the experiment. The subject of the experiment is enclosed in the inner chamber into which fresh air is continually being drawn by means of a flue where a gas burner is kept alight. The whole apparatus is suspended from the ceiling by a pulley, and balanced with a weight. It might be recollected that Marcet's calorimeter which we made use of, consists of a wooden chamber covered with felt inside and out, enclosing another made of sheet copper, carefully polished inside, with an air space between them. The size of the inner chamber is sufficient to admit of a person sitting down comfortably on a chair, with free elbow and head room. Two ventilators (agitators), worked by electric motors, constantly mix up the air inside the calorimeter, while the air from one of these ventilators impinges on a trough full of ice; the water from the melted ice is collected in a flask, and its temperature read at the end of the experiment.

Thermometers with stems projecting above the chambers show the temperature of the air in the copper chamber and the annular space, and another gives the temperature of the copper walls; these thermometers are divided into fiftieths of a degree centigrade. For further particulars we beg to refer to the original paper. The amount of heat evolved is easily calculated from the weight of ice melted, the temperature of the resulting water, and the change of temperature of the chamber, the annular space, and the copper.

Test experiments, made by means of the combustion of hydrogen

* "A Calorimeter for the Human Body," by W. Marcet, 'Roy. Soc. Proc.,' vol. 63.

† '*Travaux* du Laboratoire de Marey,' 1878–79.

gas, gave a figure (34,428 calories) almost identical with that found by Favre and Silbermann (34,462).

After having used that instrument towards the estimation of the heat given out in a state of rest,* it appeared to us of interest to apply it to the determination of the heat emitted under exercise, so as to obtain finally a figure for the mechanical energy of the human machine, or, as termed by Verdet in his book on the 'Mechanical Theory of Heat,'† the economic coefficient of the human machine.

In 1861 Helmholtz, making use of Edward Smith's experiments on the treadmill, calculated by an ingenious mode of argument what ought to be the value of this coefficient. He reasoned in the following way‡ :—A man in a state of repose emits exactly, in one hour, the amount of heat that would be required theoretically to raise his body uphill to a height of 522 m., which he looks upon as a fair rate of climbing in one hour; but, in order to accomplish this work, he would expire five times as much CO_2 as in the state of repose, and, therefore, produce five times as much heat, consequently the relation is 1 : 5, and therefore the economic coefficient would be 1/5.

Following this mode of argument, it will be readily seen that the supposed subject of this experiment, taking as an average weight 65 kilos., would have to emit in the state of rest 80·2 cals. per hour, because, on multiplying 80·2 by 423 (the mechanical equivalent of heat), the result would give 33,925 kilo.-metres; on the other hand, a man weighing 65 kilos. while raising his body by 522 m., would effect an amount of work = 33,930 kilo.-metres. Therefore the heat emitted in a state of rest in one hour would be exactly the same as the theoretical heat necessary to raise the body 522 m.; but as (according to Helmholtz) Edward Smith says it is necessary to give out five times more CO_2 than when at rest, to do that work, the coefficient will be 1/5.

This argument can be criticised as follows :—

1. Most authors give a little over 100 cals. per hour as the mean amount of heat emitted by a person in a state of repose, while, from ninety-two experiments on three different persons, we found from 79·056 cals. to 137·636 cals. with a mean of 102·260 cals., these figures being very far from 80·2, which are required to fit the present theory.

2. An ascent of 522 m. (1712 feet) in an hour is more than anybody can do going uphill; such exercise might perhaps be kept up for a minute or two, but the average height a man can ascend is usually put down at 1000 feet per hour (perhaps 1200 feet). It is difficult to say what amount of air a man would breathe during such exercise, but it

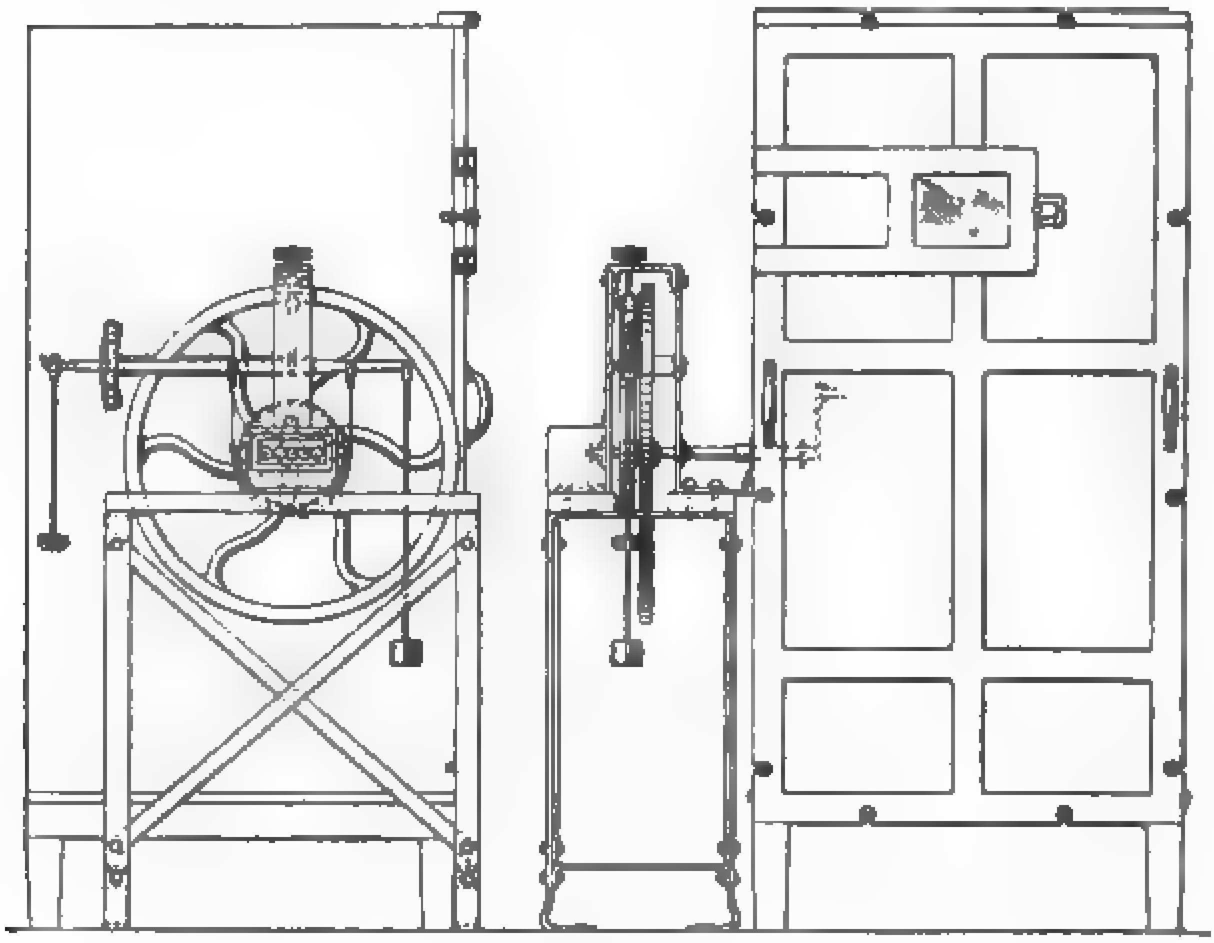
* 'Roy. Soc. Proc.,' vol. 63, p. 242.

† 'Verdet,' vol. 2, p. 250.

‡ "On the Application of the Law of the Conservation of Force to Organic Nature." Helmholtz. 'Proceedings of the Royal Institution,' vol. 3, p. 355, 1862.

would probably happen that if a person could climb at the rate of 522 m. per hour, as Smith did on the treadmill, then he would expire about five times as much CO_2 as in the state of repose; there is no reference to the oxygen absorbed in addition to the carbonic acid expired. From these considerations it will be understood how Helmholtz obtained the figure accepted in a general way as the value of the efficiency of the human body, and which is close to the figure we have found.

In order to determine experimentally the heat given out under a definite exercise, and ascertain that utilised in the work done, a brake was used, being a modification of that known to engineers as Pronié's. This brake (see diagram) consists of an iron fly-wheel resting on an iron stand and worked by a handle projecting inside the calorimeter through a stuffing box; a counter registers the number of revolutions throughout the experiment. The wheel is brought into contact with a semicircle of hard wood in sections fastened to a strap; by tightening this strap the semicircle of wood can be pressed more or less against the wheel, and thus the friction can be regulated.



On turning the wheel pressing against the wooden blocks, the tendency would be to give the blocks a revolving motion, but the force applied, instead of carrying them round and round, causes the wheel to slip over their surface, and in doing so to exert a degree of friction sufficient to raise a lever weighted at the end, and maintain it in a *horizontal* position as long as the wheel is rotated.

Therefore the power necessary to overcome the friction is exactly balanced by the weight at the end of the lever, and the work done will be a function of the weight multiplied by the length of the lever, or may be expressed as the work done in raising the weight by means of a cord wound round a drum, having for its radius the distance between the suspension of the lever and the end of the beam where the weight is hung. It should be understood that the beam is balanced by a counterpoise before the weight is suspended to its extremity.

The only objection that could be made to the use of this instrument might be the friction to overcome on turning the wheel; but there is no appreciable friction, the wheel revolving on ball-bearings like those of a bicycle. The weight can be altered, and it will be readily found that if a person works the wheel as hard as he can for a given time, by doubling the weight at the end of the lever the number of revolutions he can make in that same time will be half that registered with the lighter weight.

The experiment in the calorimeter is made as follows:—

The subject is shut up in the chamber as usual, and there he remains for thirty minutes quite quiet; during that time the temperatures are recorded, and the water from the melted ice is collected, thus the heat given out in the state of repose is determined. Then the person is let out of the calorimeter, and the chamber left full open for its ventilation, and about twenty minutes later he returns into the chamber. At once the work of turning the handle begins, to be continued for about half an hour. Then a rest is taken for another half hour inside the calorimeter, and the experiment is over.

By so doing the heat emitted in the state of rest is first determined; this is followed by the determination of the heat given out when at work and while the body is returning to its normal state of rest. It might be thought that the amount of work done had better be determined by first working the wheel perfectly free and therefore effecting no work, and then putting on the brake while the lever is weighted and doing a certain amount of work; the heat emitted in the first case would be subtracted from that given out in the second. This plan, however, is not admissible, because, at any rate, no work can be done without moving the arm, so that the arm is a necessary factor of the work.

The first question was to ascertain whether the mean relation of the oxygen absorbed under work to the calories emitted was the same as the corresponding mean relation in the state of rest. In order to settle this point, the CO_2 emitted and O absorbed were determined while in the calorimeter, together with the calories, both when sitting quiet and during exercise, the relation at rest having been fully investigated in our last paper and found to be 1 : 4·000.*

* It was stated in the previous paper that Hirn had found that while sitting quiet, 1 gram of oxygen gave rise to a mean of 5·22 calories.

The experiment was carried out by inspiring the external air through a nasal tube and expiring through a mouth-piece into bell-jar receivers suspended over water.*

The following are the results obtained:—

Relation between the Oxygen absorbed and the Heat emitted during Work.

	Total calories during work.				
	Calories emitted during work.	Number of turns of wheel, 1 turn = 1·471 kilogram-metres.	Calories absorbed during work (theoretical).	Oxygen absorbed during work.	Calories for 1 gram oxygen absorbed.
				grams.	
W. M.	51·835	836	2·904	17·236	3·176
	52·776	812	2·820	18·698	2·978
	51·313	807	2·803	16·810	3·219
	52·367	771	2·678	18·875	2·916
				Mean....	3·071
R. B. F.	145·147	2870	9·967	48·348	3·208
	123·120	2306	8·009	36·992	3·545
	124·283	2147	7·457	37·190	3·542
	106·305	2102	7·300	35·246	3·223
	124·301	2275	7·901	37·930	3·486
				Mean....	3·401
E. R.	64·094	1097	3·810	24·767	2·742
	68·129	1041	3·616	21·012	3·414
	65·057	945	3·282	22·595	3·025
	58·554	839	2·914	19·455	3·160
	135·488	2174	7·550	42·704	3·349
	141·815	2170	7·536	44·029	3·392
	130·002	2379	8·262	46·343	2·984
	131·698	1976	6·863	44·821	3·091
	136·970	2050	7·119	43·339	3·325
	137·584	1969	6·838	43·496	3·320
	140·579	2198	7·634	45·715	3·242
	140·192	2064	7·168	41·139	3·582
	136·936	2092	7·266	42·377	3·408
	150·881	2151	7·470	43·033	3·676
				Mean....	3·265

Consequently the oxygen absorbed does not follow exactly the amount of heat produced; but a number of experiments for separate individuals show in each case means somewhat approximating each other, though less obviously than in the state of rest.

* The persons who submitted to these experiments had practised this mode of *breathing for* a considerable time, and it had become with them quite natural.

The present inquiry includes twenty-three experiments :—

	Gram O.	Calories.
The first set of four experiments on W.M. gave......	1	3·071
The second set of five experiments on R. B. F. gave	1	3·401
The third set of fourteen experiments on E. R. gave	1	3·265
Mean	1	3·246

Therefore it will be readily seen that 1 gram of oxygen will give rise to more heat in a state of rest (1 : 4·000) than under exercise (1 : 3·246), and it cannot be admitted that the same mean amount of heat is produced by a given weight of oxygen absorbed in repose and under exercise; Hirn observed a similar occurrence. It may therefore be concluded that the human body in the state of rest makes a more efficient use of its oxygen than it does under exercise in the proportion of about 4·000 : 3·246.

The next, and perhaps most important subject for consideration, is the efficiency or economic coefficient of the human machine.

The efficiency of the body as a machine is the relation between the theoretical heat corresponding to the work done* and the actual amount of heat that the body requires to do this work. According to this definition the result sought for in our inquiries was obtained by dividing the theoretical amount of heat necessary for the work done in each experiment by the heat given out during the work, less the normal heat emitted in the same time plus the theoretical heat necessary for the work. This statement can be expressed in the form of a formula.

If

E = Efficiency of the human machine.
T = Theoretical calories necessary for the work done,
C = Heat emitted during the work,
c = Heat emitted in a state of rest,

then

$$E = \frac{T}{C - c + T}.$$

The weight suspended from the end of the lever forming part of the brake was 485 grams, and knowing the length of the lever (48·275 cm.) it was easy to calculate that each turn of the wheel performed an invariable amount of work = 1·471 kilogram-metres.

* Mechanical equivalent of heat multiplied by work done in kilogram-metres.

Table showing the Efficiency (Economic Coefficient) of the Human Machine.

W. M. under Experiment.

Calories emitted in state of rest.	Calories emitted during work, followed by rest for an equal time.	Revolutions of the wheel.	Work done in kilogram-metres.	Calories, theoretical, corresponding to work done.	Heat utilised, *i.e.*, efficiency.
½ hour.					
39·872	51·835	836	1230	2·904	0·195
40·786	52·776	812	1194	2·820	0·190
40·261	51·313	807	1187	2·803	0·203
43·369	52·367	771	1134	2·678	0·229
52·703	65·801	704	1036	2·445	0·157
59·135	70·012	894	1315	3·105	0·222
45·127	59·715	861*	1035	2·443	0·143
Mean 45·893	57·688		1162	2·743	0·191
1 hour.					
87·010	104·720	1556	2289	5·405	0·234

R. B. F. under Experiment.

Calories emitted in state of rest.	Calories emitted during work, followed by rest for an equal time.	Revolutions of the wheel.	Work done in kilogram-metres.	Calories, theoretical, corresponding to work done.	Heat utilised, *i.e.*, efficiency.
½ hour.					
43·745	85·717	1670	2457	5·801	0·121
43·887	70·311	1269	1867	4·408	0·143
45·087	70·643	1003	1475	3·484	0·120
50·284	60·269	1023	1505	3·552	0·261
36·839	59·121	911	1340	3·164	0·124
45·165	75·362	1228	1806	4·265	0·124
42·708	56·927	620*	1777	4·196	0·228
Mean 43·959	68·336		1747	4·124	
1 hour.					
93·594	113·428	1811	2664	6·291	0·241
90·166	135·607	2176	3201	7·559	0·142
97·786	136·097	2278	3351	7·913	0·171
97·760	140·845	2570	3780	8·928	0·172
93·512	133·238	2299	3382	7·985	0·167
87·408	128·783	2694	3963	9·357	0·184
101·696	141·524	2509	3691	8·714	0·180
83·684	145·147	2870	4222	9·967	0·140
80·976	123·120	2306	3392	8·009	0·160
85·210	124·283	2147	3158	7·457	0·160
80·496	106·305	2102	3092	7·300	0·220
104·244	124·301	2275	3346	9·901	0·283
Mean 91·378	129·390		3437	8·115	0·176

* Double weight on brake.

E. R. under Experiment.

Calories emitted in state of rest.	Calories emitted during work, followed by rest for an equal time.	Revolutions of the wheel.	Work done in kilogram-metres.	Calories, theoretical, corresponding to work done.	Heat utilised, *i.e.*, efficiency.
½ hour.					
47·283	58·554	839	1234	2·914	0·205
51·963	65·057	945	1390	3·282	0·200
52·207	68·129	1041	1531	3·616	0·185
46·746	64·094	1097	1614	3·810	0·180
61·975	83·010	1055	1552	3·664	0·148
53·801	88·244	1248	1542	4·335	0·112
Mean 52·329	71·181		1477	3·603	
1 hour.					
136·608	162·132	2482	3651	8·622	0·252
130·208	178·722	2582	3798	8·969	0·156
115·868	172·443	2551	3753	8·861	0·135
133·392	194·626	2689	3956	9·339	0·132
137·636	177·412	2545	3744	8·839	0·182
128·578	177·915	2340	3442	8·127	0·141
107·355	137·584	1969	2896	6·838	0·184
102·545	140·579	2198	3233	7·634	0·167
114·885	140·192	2064	3036	7·168	0·221
107·783	136·936	2092	3077	7·266	0·200
113·451	150·881	2151	3164	7·470	0·166
Mean 120·755	160·857		3432	8·103	0·174

E. F. under Experiment.

Calories emitted in state of rest.	Calories emitted during ½ hr. work, followed by ½ hr. rest.	Revolutions of the wheel.	Work done in kilogram-metres.	Calories, theoretical, corresponding to work done.	Heat utilised, *i.e.*, efficiency.
1 hour.					
101·090	121·099	1892	2783	6·572	0·247
120·418	147·723	2242	3298	7·788	0·222
119·822	157·280	2287	3364	7·944	0·175
101·758	153·264	2335	3435	8·111	0·136
102·302	152·476	2170	3192	7·538	0·131
105·774	149·040	2302	3386	7·997	0·156
110·220	130·701	1499	2205	5·207	0·203
101·752	123·203	1527	2246	5·303	0·198
102·582	164·413	2445	3597	8·492	0·121
104·642	119·885	1761	2590	6·116	0·286
112·140	144·660	2007	2952	6·971	0·176
108·557	168·365	2444	3595	8·489	0·124
101·160	161·014	2427	3570	8·430	0·123
96·078	133·818	2008	2954	6·974	0·156
119·860	144·226	2046	3010	7·104	0·226
120·800	148·591	2114	3110	7·342	0·209
108·056	144·985		3080	6·684	0·181

M. (Woman) under Experiment.

Calories emitted in state of rest.	Calories emitted during ½ hr. work, followed by ½ hr. rest.	Revolutions of the wheel.	Work done, in kilogram-metres.	Calories, theoretical, corresponding to work done.	Heat utilised, *i.e.*, efficiency.
1 hour					
85·546	116·644	2303	3388	7·999	0·204
84·036	127·086	2543	3741	8·832	0·170
64·140	108·836	2522	3710	8·759	0·164
78·750	110·703	2231	3282	7·748	0·195
78·576	114·899	2370	3486	8·231	0·185
78·094	109·205	2363	3476	8·207	0·209
77·374	102·955	2165	3185	7·520	0·227
Mean 78·074	112·904		3467	8·185	0·193

The five tables include sixty-seven experiments, being all those made (seventy-one in number) with the exception of four; these, which were undertaken on four consecutive days, when the work was resumed last autumn, yielded for the value of the efficiency, at any rate in three cases, figures obviously much too high, and which have been omitted on that account; they were the following:—for E. R., 0·462, 0·231, 0·384, 0·363. These irregularities are apparently due to some accidental mis-estimation of the water from the melted ice.

In every case, as will be readily seen from the foregoing tables, the amount of heat emitted under exercise is largely in excess of that given out in a state of rest during the same time; this excess, in the case of the five persons under experiment, amounted to

In thirty minutes.	Per cent. of normal.	Calories excess + theoretical for 1000 kilogram-metres.
11·795 cal. for W. M.	25·7	12·511
24·377 „ „ R. B. F.	55·5	16·314
18·852 „ „ E. R.	36·0	15·203
In one hour.		
38·012 cal. for R. B. F.	41·6	13·421
40·102 „ „ E. R.	33·2	14·046
36·929 „ „ E. F.	34·2	14·159
34·830 „ „ M.	44·6	12·407

From a consideration of the foregoing tables it will be seen that the inquiry as to efficiency, made on five different persons, yielded the following results:—

	Age.	Weight.	Occupation.	Efficiency.	No. of Experiments.
		kilos.			
W. M.	69	57·9	Laboratory work	0·191	7
R. B. F.	28	53·0	" "	0·176	19
E. R.	29	80·4	" "	0·174	17
E. F.	16	..	" "	0·181	16
M. (woman)..	47	..	Charwoman	0·193	7
			Mean	0·183	66

Therefore the maximum mean efficiency was 0·193 in the case of the woman from seven experiments, and the minimum mean efficiency was 0·174 for E. R. from seventeen experiments; the general mean from five different persons was 0·183.

The general results obtained from this inquiry can be summarised as follows:—

(1) There is no fixed relation per individual experiment between the oxygen absorbed under excercise and the corresponding heat emitted, although the mean for each person somewhat approximates a constant figure which is 1 to 3·246. Considering that in the state of rest we found the corresponding ratio to be 1 to 4·000, it may be concluded that the oxygen is better utilised for the production of heat in a state of rest than under exercise.

(2) There is a marked excess of heat over normal given out under exercise, this excess (+ theoretical heat) produced in doing a definite amount of work (say 1000 kilogram-metres) varies for each of the five persons under experiment.

(3) The efficiency, or economic coefficient, for the five persons under experiment varied from 0·193 to 0·174 with a mean of 0·183; or 18·3 per cent. of the excess heat produced + the theoretical heat corresponding to the work done. This is a little less than a fifth.

"Some Experiments bearing on the Theory of Voltaic Action." By J. Brown. Communicated by Professor Everett, F.R.S.

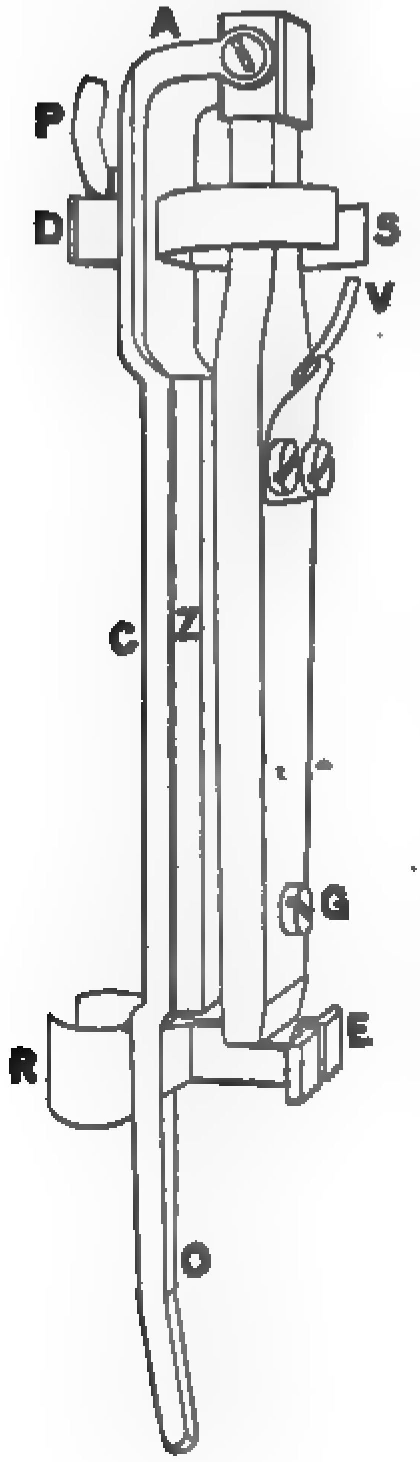
Received February 4,—Read February 23, 1899.

In former papers on the "Theory of Voltaic Action,"* I have adduced evidence in support of the view that the difference of electric potential observed near the surface of two metals in contact is caused, or at all events mainly influenced, by the chemical activity of films

* 'Phil. Mag.,' vol. 6 (1878), p. 142; *ibid.*, vol. 7 (1879), p. 109; 'Roy. Soc. Proc.,' vol. 41 (1886), p. 294.

condensed on their surfaces from the atmosphere (vapour or gas) in which the metals are immersed. From this view it would naturally follow that, if all chemically active films and all atmosphere competent to produce them could be removed, the difference of potential would disappear also. This inference has been pointed out and acted upon by previous experimenters, as is well known; but it appeared to me that no certain test of the theory could be obtained in the way they suggested without elaborate precautions as regards details; and the experiments here to be described were undertaken in the hope that more definite results might follow greater care in the work.

The method adopted was to enclose a copper-zinc volta condenser in a glass tube containing nitrogen at a small pressure, together with metallic potassium and sodium, the expectation being that these metals would absorb any remaining water vapour or other agents (compounds of oxygen, &c.) that could exert chemical action on the zinc.



The condenser is represented in the figure. Its plates are 101 mm. long by 47 mm. wide. The copper plate C has prolongations at both ends, to which are attached the fittings D E, and the springs R S, for the support of the whole system in its glass tube. To the prolongation A of the copper plate is hinged, on pointed screws, a brass sleeve, into which is cemented the glass plate G, as an insulating support for the zinc Z. The free end of the glass plate hangs in a notch in the fitting E, and can move to the extent of the width of this notch, so as to separate the plates when the condenser with its tube is tilted over till the zinc falls away from the copper. Two curved springs of strip steel are placed between the glass and the zinc, to keep them apart; while they are held together by three screws, passing through the glass, and about half way through the zinc. These screws also serve to keep the copper and zinc, when in their position of nearest approach, at the uniform distance of about 0·03 mm.

The surfaces of the places were made true by careful filing, scraping, and testing by a surface plate. Platinum-tipped contact springs, P V, are provided to make connection with platinum wires sealed into the containing tube, which is of lead glass,

53 mm. diameter, and about 30 cm. long, with a leading tube attached on one side. The measurement of the difference of potential was made by a well-known zero method. The deflection given by a quadrant electrometer, on separating the condenser plates, was annulled by connecting the plates to points in a circuit of variable high resistance, containing a large gravity Daniell cell; the electromotive force required thus to annul being taken as equal and opposite to the difference of electrostatic potential at the plates. Three similar sets of measurements were made with this apparatus, continuing after it had been sealed up respectively six months, one and a half years, and seven and a half years. In the intervals between observations the plates remained out of metallic contact, and were kept, in Experiment I at their greatest distance apart; in III generally at their least. My notes of Experiment II are not clear on this point.

Experiment I, started December 12, 1888.—The plates having been cleaned with fine glass paper, the condenser was slipped into its tube. A small porcelain cup of phosphorus pentoxide was introduced, in order to dry out the interior, and the tube was then temporarily closed. The difference of potential was then found to be

0·74 volt.

The end of the main tube was then sealed at the blowpipe. The tube was exhausted through the side tube by a Sprengel pump, then filled with nitrogen, again exhausted, and then refilled with nitrogen. The condenser plates were now found to be in metallic contact, presumably due to the accidental sucking in of some minute globule of mercury from the pump. In three days this contact ceased. About 3 grams of potassium and 1 gram of sodium, in small pieces, were now inserted by the side tube. (My notes do not state that the capsule of phosphorus pentoxide was removed before closing the tube, but probably it was.) The tube was again, December 17, exhausted to 3 mm., and refilled with nitrogen, when the difference of potential was found to be about

0·64 volt.

The tube was finally exhausted to 4·5 mm., and the side tube was sealed off, the difference of potential being

0·61 volt.

The following observations were then made at the intervals noted in days after thus starting the experiment:—

Days...	13	25	27	30	61	106	173	181
Volt ...	0·56	0·52	0·55	0·51	0·47	0·34	0·32	0·33

It remained to ascertain whether the fall in potential-difference was due to the gradual absorption of chemically active matters by the potassium and sodium, or to the well-known effect of gradual tarnishing of the zinc surface. If, on admitting air and moisture to the tube, the potential-difference increased, the former alternative would be indicated, and the absence of such increase would indicate the other alternative.

Before testing this point, it was thought desirable to ascertain whether the pressure originally in the tube had changed. To measure the pressure, the sealed end of the leading tube was joined by a rubber tube and mercury seal to the Sprengel pump, which was worked till the pump gauge showed a pressure of about 2 mm. The end of the leading tube was then broken off in the inside of this rubber tube, a notch having previously been filed to facilitate breaking. The pump gauge then fell, and ultimately stood at 90 mm. pressure, showing that a considerable amount of gas had been evolved in the tube during its six months' trial.

The leading tube was now removed from the pump, and air admitted; air was also blown in by the mouth, to introduce moisture. The difference of potential at once rose to

0·39 volt, and later to 0·48 volt.

On taking out and examining the condenser, the zinc was found to be tarnished at the edges, but not much in the middle of its surface; the sodium was scarcely altered, but the potassium had a thick coat of, no doubt, oxide or hydrate, covering a core which burned on water.

Experiment II, started December 9, 1889.—This was intended to be practically a repetition of Experiment I. In closing the end of the main tube, a considerable amount of fumes and moisture from the gas blowpipe was observed to get into it, which may have affected the condition of the zinc surface. The moisture was removed by warming the tube and washing out with air. 8 grams of potassium were inserted, but no sodium. The nitrogen pressure before sealing off was 5 mm. After sealing off, the difference of potential was found to be

0·70 volt,

and fell thereafter more or less regularly for a year and a half, when, on June 9, 1891, it had diminished to

0·52 volt.

On opening the tube this value did not sensibly change. The fall in difference of potential was therefore probably due to tarnishing of the zinc merely. The potassium in the tube was very little altered.

Experiment III, started June 15, 1891.—The arrangement was the

same as in Experiment I, except that, after the tube had been exhausted and finally sealed off, the 9 grams of potassium and 3 grams of sodium which had been introduced, were fused together, forming the alloy that is liquid at ordinary temperatures. The difference of potential was at first about

0·75 volt.

During the first year, the whole tube (except when being examined) was kept immersed in a bath of petroleum, to prevent leakage of air, in case of minute imperfections at the sealed-in wires or elsewhere. The difference of potential on July 22, 1892, was about

0·67 volt.

The tube, no longer kept in petroleum, was examined occasionally for the next six and a half years, till November 4, 1898, when it was opened. The pressure had risen to about 59 mm. of mercury. The difference of potential just before opening was about

0·49 volt;

and after opening and blowing in there was little appreciable change in this value. If anything, it seemed rather lower; though the rapid tarnishing of the potassium-sodium alloy, when a new surface of it was exposed, indicated the presence of an ample amount of oxidising medium. The decrease of potential difference, in this case also, was therefore probably due to tarnishing of the zinc surface. The zinc was almost as bright as when enclosed seven and a half years before; but polishing a small portion of its surface with glass paper showed that a slight film had formed.

Of the three experiments, the first is the only one that lends any support to the hypothesis they were designed to illustrate. The laboratory notes show no difference between the first experiment and the other two, beyond what may be gathered from the foregoing account. It seems unlikely that the three days of accidental metallic contact in the first experiment or the distance apart of the plates in intervals between observations can have affected the result; and I am unable to suggest any other explanation except the possibility that the phosphorus pentoxide was left in the tube as well as the potassium and sodium. I found on a previous occasion,* that when this substance was enclosed with a copper-zinc pair, so as to dry the air surrounding the pair, the difference of potential fell in 134 days by one-sixth of its first value, and rose to its original amount immediately on admission of the ordinary atmosphere. In No. III certainly no phosphorus pentoxide was enclosed, and there is no mention of it in my notes of No. II.

* 'Roy. Soc. Proc.,' vol. 41 (1886), p. 305.

Though the experiments cannot be quoted as confirming the chemical hypothesis, which I still think to be supported by an overwhelming weight of evidence, it has been thought worth while to describe them, if only to show the extreme difficulty of eliminating the last traces of active matter from the gas employed. That this is the real difficulty in the way of obtaining positive results is well illustrated by the ingenious experiments of C. Christiansen.* He found, among other things, that when the metal (of a pair) near which positive potential is usually observed is exposed, for a minute fraction of a second, to an inactive gas, such as hydrogen, the observed potential difference is very much smaller than when the exposure lasts for a considerable time. The metal exposed by Christiansen was a jet of liquid amalgam, flowing from a drawn out glass tube. Its surface was thus perfectly clean, and the time of exposure to the surrounding gas was merely the interval between the instant at which the amalgam left the nozzle and that at which it broke into drops. The difference of potential observed, when carbon was opposed to a jet of zinc amalgam in hydrogen in this manner, was only 0·15 volt; while in air it was 0·89 volt. If more time had been allowed, the impurities in the hydrogen would have diffused in larger quantity towards the zinc, and given a larger effect, similar in character to that observed in my experiments, where the metals are exposed to the gas for a period amply sufficient for all such action.

"Deposition of Barium Sulphate as a Cementing Material of Sandstone." By FRANK CLOWES, D.Sc., Emeritus Professor, University College, Nottingham. Communicated by Professor H. E. ARMSTRONG, F.R.S. Received February 7,—Read February 23, 1899.

Some years ago I described the occurrence of a peculiar sandstone over a large area in Bramcote and Stapleford, near Nottingham.† The sandstone was remarkable for its high specific gravity, and chemical analysis, supported by microscopical examination, proved that the high specific gravity was due to the existence in the sandstone of a large proportion of highly crystalline barium sulphate. In the rock itself the percentage of the sulphate varied from 33·3 to 50·1: and it evidently served as the binding or cementing material which held the sand grains together. The occurrence of this sandstone was stated by geologists to be unique in the United Kingdom.

Mr. J. J. H. Teall made an examination of a portion of the sandstone, and stated that after breaking up a portion of the rock, he easily

* 'Wied. Ann.,' vol. 56 (1895), p. 644.

† 'Roy. Soc. Proc.,' vol. 46, p. 363.

effected a separation of the sulphate from the sand by shaking the powder about in water: the small cleavage flakes thus obtained gave the optical characters of crystallised barium sulphate. Mr. Teall further stated that the barium sulphate occurred in large irregular crystalline patches, which included the sand grains; the sand grains, therefore, interrupted the reflections from the cleavage surfaces of the barium sulphate, giving rise to the appearance generally known as "lustre mottling" in petro-graphic literature.

I had noted this irregular distribution of the barium sulphate. In some parts of the rock the sulphate occurred in reticulated veins inclosing small patches of more or less loose sand grains; while in other parts of the rock the sulphate occurred in spherical or oval masses, between which looser sand was interspersed: occasionally, however, the barium sulphate was uniformly distributed.

The appearance presented by the weathered surface of the rock varied much according to the mode in which the resistant sulphate was distributed. When it was uniformly distributed, it formed an almost complete protection against weathering: this was seen on the cap of the great pillar of this rock, which is locally known as the "Hemlock Stone." The reticulated distribution of the sulphate caused the surface of the weathered rock to present a fretted surface, with the thin veins of sulphate projecting from the surface. When the sulphate had bound together spherical or oval masses in the substance of the sand, these were left in pebble-like forms as soon as the loose sand had been washed out from between them; and the resulting layers of loose sand, inclosing the rounded masses of sand bound together by the sulphate, had been not unnaturally classed by the geologists who had visited the district, as pebble-beds.

In discussing various ways in which this barium sulphate might have been deposited, I drew attention to the frequent deposition of barium sulphate from colliery water in the neighbourhood of Newcastle-upon-Tyne, and described some of these deposits.* And I further pointed out that Dr. Bedson had shown† that barium chloride was a common constituent of the colliery waters in the district in which these barium sulphate deposits occur: and that it was present to the extent of 137·2 parts per 100,000 in some of these waters. It was evidently only necessary that water containing sulphuric acid, or a soluble sulphate should mingle with the barium chloride water in order to explain the deposition of barium sulphate in the positions in which it was found. In colliery districts a frequent source of ferrous sulphate and of sulphuric acid is found in the iron pyrites in the beds of coal and shale. And I suggested that the constant occurrence of fine veins of calcium sulphate throughout the sandstone of the Nottingham district would account

* 'Roy. Soc. Proc.,' June, 1889.

† 'J.S.C.I.,' vol. 6, p. 712.

for a sulphate finding its way into the water which had been in contact with the rock. But in the Nottingham district all evidence of barium chloride in solution was wanting.

The occurrence of barium chloride in water from an artesian boring at Ilkeston has, however, recently been pointed out by Mr. John White, and he has described the nature of the strata through which the boring passed, and the results obtained by him in the chemical examination of the water, in 'The Analyst' (February, 1899). The Ilkeston boring has been made in the immediate neighbourhood of the Bramcote and Stapleford sandstone which contains the large proportion of barium sulphate. Since the barium chloride is found to the extent of 40·7 parts per 100,000 in the water from this boring, and seems to be a normal constituent of the water, it would appear that soluble barium salts are still abundant in the district, and may therefore have given rise to the deposition of the barium sulphate in the original sand beds. The crystallisation of the sulphate around the sand grains would then cause it to act as a compact, insoluble cementing material.

It is worthy of note that one of the samples of water from the boring contained a small amount of barium in the presence of a large amount of sodium carbonate; in this case the barium must therefore itself have been present as bicarbonate.

Water containing barium chloride to the extent of about 9 grains per 100,000 has recently been found at Llangammarch in Breconshire.

Since the publication of my original paper on the occurrence of barium sulphate in the Bramcote sandstone, I have continued my examination of samples of sandstone from the basement of the pebble beds of the Bunter, with the object of ascertaining whether the occurrence of barium, either as sulphate or in other forms of combination, was characteristic of the sandstones of that geological period. I have thus far failed to find any similar rock to that at Bramcote, and it therefore seems probable that the occurrence of barium sulphate, although it extends over a very extensive area at Bramcote and Stapleford, must be looked upon as being due to purely local causes.

[*February* 22.—Mr. J. Lomas, in a letter dated 20th instant, draws my attention to a paper read by him and Mr. C. C. Moore before the Liverpool Geological Society, on February 8, 1898, in which the authors draw attention to the occurrence of large proportions of crystallised barium sulphate in triassic sandstones at Prenton and Bidston. Mr. Lomas had previously mentioned the presence of the sulphate in the Bidston sandstone thirteen years ago in a paper to the above Society.

In different specimens of the sandstone the percentage of the sulphate varied from 12·4 to 33·8 per cent. It is colourless and highly

crystalline, and is adherent to the sand grains in such a way as to show that it has been deposited *in situ* subsequently to the sand grains. Mr. Lomas states that the occurrence of barytes in the trias is fairly common, and mentions the following localities, in which its presence is well known:—Beeston, Alderley Edge, Oxton, Storeton, and Peakstones Rock, Alton. The sulphate is also stated to occur at West Kirby, in Cheshire, and elsewhere as a joint filling, the joints often standing out from the surface of the rock, owing to the resistance of the sulphate to weathering.]

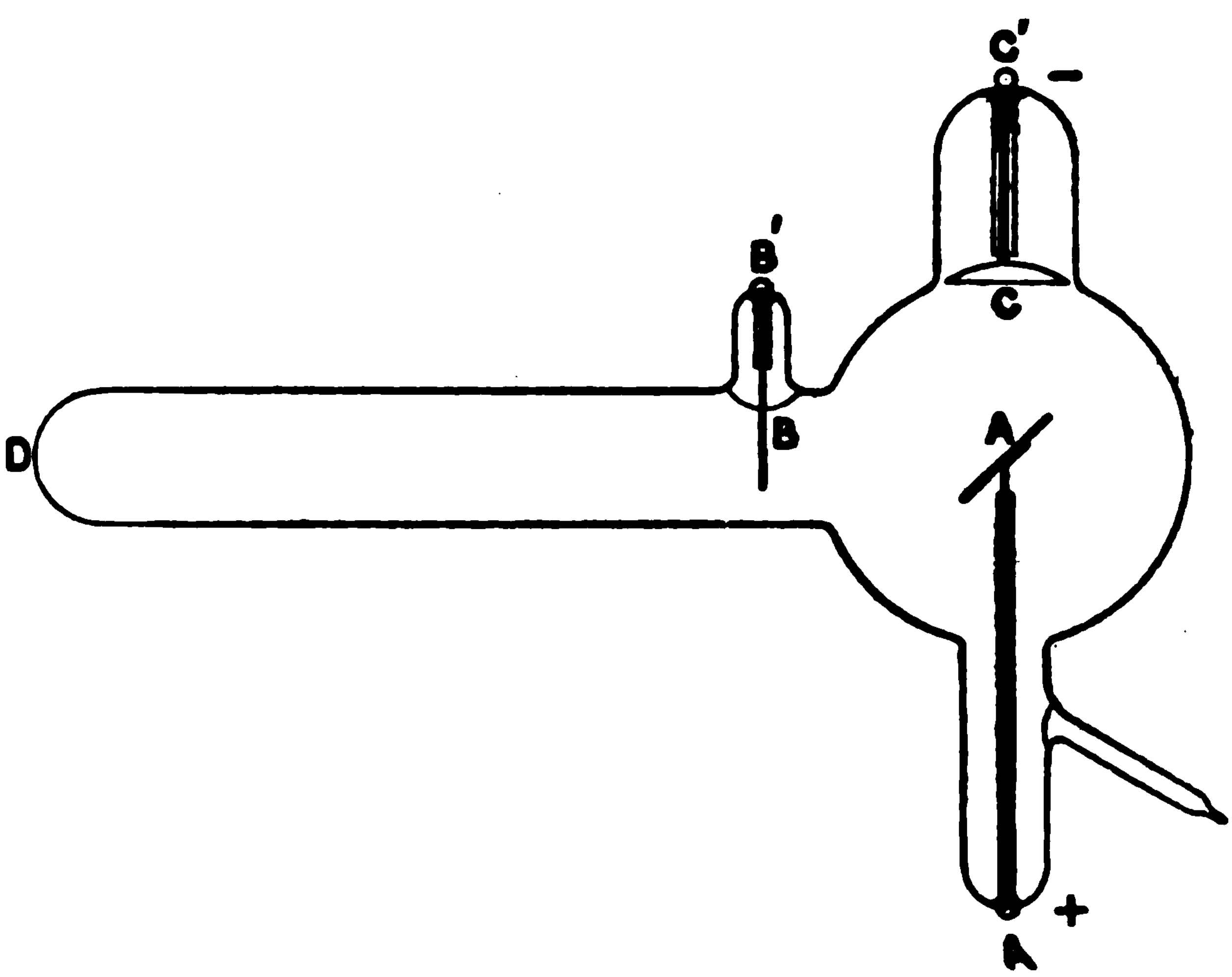
"On the Reflection of Cathode Rays." By A. A. CAMPBELL SWINTON. Communicated by LORD KELVIN, F.R.S. Received January 25,—Read February 9, 1899.

Preliminary.

There being apparently some doubt as to the exact nature of the rays, named by Professor S. P. Thompson paracathodic rays,* which in a Crookes tube of the focus type proceed from the front surface of the anti-cathode, and cause the green fluorescence of the glass, the writer has made the following investigations:—

Firstly, in order to determine the magnetic deflectibility of the paracathodic rays, a tube was constructed as shown in fig. 1, in which C is the cathode, A the anti-cathode and anode, and B an aluminium wire

FIG. 1.



* 'Phil. Trans.,' vol. 190, pp. 480—483.

sealed into the glass of an elongated annex. The tube was exhausted to about 0·000005 atmosphere, and the arrangement was such that the paracathodic rays proceeding from A, cast a sharp shadow of B upon the glass at D. The distance from B to D was made long so that a small horse-shoe magnet, held so as to embrace the annex between B and D, would deflect the paracathodic rays without materially affecting the cathode rays passing from C to A. With this arrangement it was found that the shadow of B, cast by the paracathodic rays, was always moved by the magnet in the same direction as it would have been moved had it been cast by cathode rays proceeding from A, thus showing that paracathodic rays are magnetically deflected in the same direction as cathode rays.

This would point to the paracathodic rays consisting of negatively charged particles, as does also the fact, noted by Professor S. P. Thompson, with a somewhat similar tube, and confirmed by the writer, that when the wire B is positively or negatively charged from a separate electrical source, the consequent contraction or enlargement of its shadow at D denotes electro-static attraction or repulsion between B and the rays.

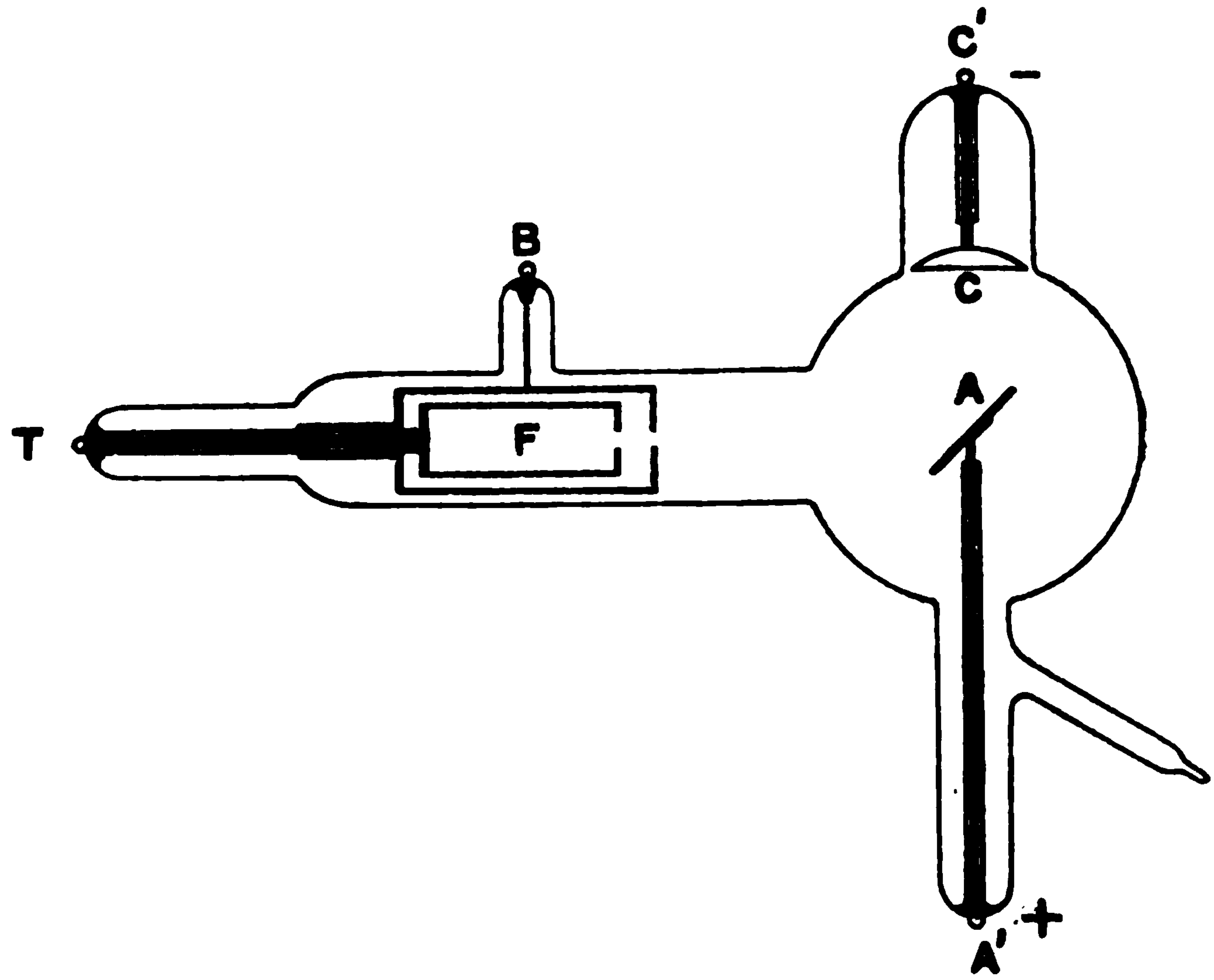
However, as previously noted by the writer,* an exploring pole immersed in the paracathodic rays, acquires a positive charge, and the wire B in the tube illustrated in fig. 1, was found invariably to have a slight positive charge, if tested with an electroscope when the tube was being used as in the first experiment described above. On the other hand, it was found that when the wire B was used as anode instead of A, the latter also acquired a positive charge, though directly played upon by the cathode rays.

It was therefore decided to determine the nature of the electrification of the paracathodic rays by means of the Faraday cylinder method employed by Perrin for testing cathode rays. It is generally agreed that this method gives more conclusive results than those obtained with exploring poles, where the effects of induction and possibly other causes appear to introduce errors.

A tube was therefore constructed as shown in fig. 2, in which C is the cathode and A is both anode and anti-cathode; F is the Faraday cylinder of brass, pierced by a small aperture through which the paracathodic rays from A can enter, and connected by means of a wire entirely enclosed in glass, with the terminal T. The Faraday cylinder is enclosed in another coaxial brass cylinder, also having an aperture facing the anticathode, and connected with the terminal B, which during the experiments was connected to earth, so as to screen the Faraday cylinder from outside influence. The Faraday cylinder was connected through T with the leaves of an electroscope, and when the tube was put into action and the paracathodic rays entered the cylinder,

* 'Roy. Soc. Proc.,' vol. 63, 1898, pp. 436—437.

FIG. 2.



it was found that the gold leaves invariably diverged with a negative charge. The divergence of the leaves was increased by connecting A to earth, and when a horse-shoe magnet was held so as to deflect the paracathodic rays, and prevent them from entering the Faraday cylinder, the closing together of the leaves showed that the cylinder no longer received any charge at all.

These experiments appear to show conclusively both that paracathodic rays are deflected magnetically in the same way as cathode rays, and also that they behave similarly to the latter in conveying a negative charge. In addition they cause green fluorescence of the glass upon which they fall, and as the writer has already shown,* they also generate Röntgen rays where they impinge upon a solid body.

Paracathodic rays appear therefore to be simply reflected cathode rays.

The Mechanical Force Exerted by Reflected Cathode Rays.

These reflected cathode rays appear however to be relatively of very feeble intensity. The amount of Röntgen rays that they generate where they strike the glass is very small; while, so far as the writer has been able to ascertain, they exert no appreciable mechanical force on the most delicately arranged radiometer mill wheels.

At one time it seemed possible that reflected cathode rays might be the cause of the inverse rotation of mill wheels placed just outside of

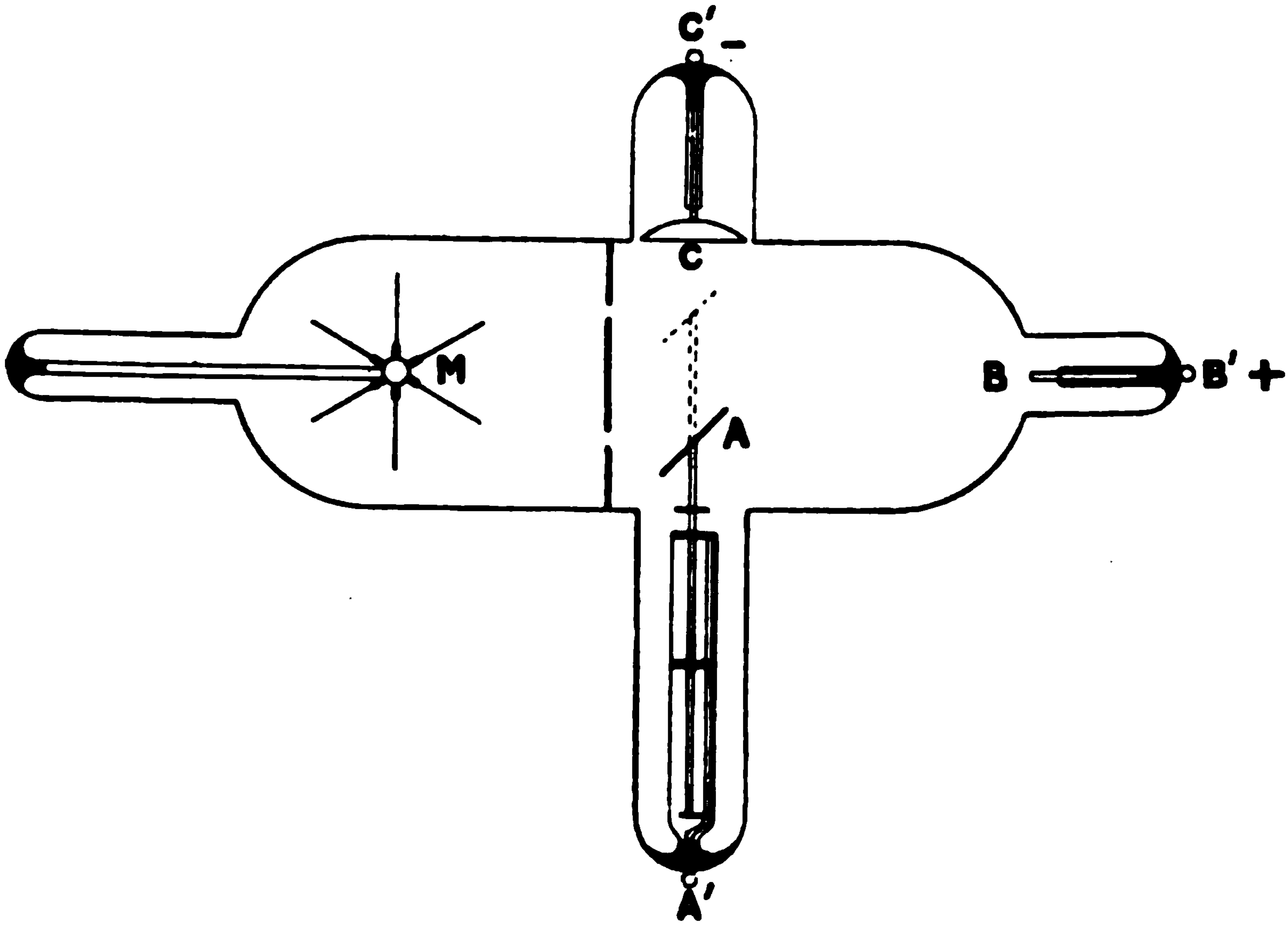
* 'Roy. Soc. Proc.,' vol. 63, 1898, pp. 436—437.

the cathode stream, described recently by the writer in two papers to the Physical Society.*

For the purpose of testing this, several experiments were made. Firstly, a tube was constructed in which the anticathode was mounted on an axis, so that by rotating it through a small angle the cathode rays could be reflected on to one side or the other of a very delicately pivoted mill wheel with mica vanes. That the rays were so reflected was apparent from the fluorescence of the glass and the shadows cast by the vanes, but the rotations of the wheel were quite inconclusive, and did not appear to have any definite relation to the direction of the reflected cathode rays.

The tube shown in fig. 3 was next constructed. In this tube there is a diaphragm of mica, that divides the tube into two portions. One portion contains the six-vaned mill wheel M, and the other the

FIG. 3.



cathode C, and an inclined anticathode A. The diaphragm is pierced with two oblong apertures, and the anticathode arranged on a sliding stem so that it can be placed to reflect the cathode rays through one or other aperture on to one side or the other of the wheel as desired. When exhausted and connected either to an induction coil or influence machine, the reflected cathode rays from A in either position passing through *the corresponding* aperture in the diaphragm, gave a distinct patch of

* 'Phil. Mag.,' October, 1898, pp. 387—395.

fluorescence on the glass beyond, throwing upon the latter a well-defined shadow of the vanes of the mill wheel. Under these conditions the wheel was found to rotate, but not in the direction anticipated; in fact, for either position of the anticathode the direction of rotation was most persistently opposite to what would be expected on the supposition that the driving force was the impact of the reflected rays. When the position of the anticathode was suddenly moved so as to reflect the rays first through one aperture and then through the other, the direction of rotation immediately reversed itself, the direction of rotation being always as though there was some attractive force between the anticathode and the particular vanes upon which the reflected rays were at the moment falling.

These and further experiments, made with another arrangement in which instead of a single bulb divided by a mica diaphragm, two separate bulbs were used, one containing the cathode and anticathode and the other the mill wheel, united by a pair of glass tubes corresponding to the apertures in the mica, appear to show that whatever may be the cause of the inverse rotation of mill wheels which are not directly acted upon by cathode rays, this is not due to the direct mechanical force exerted by the impact of reflected cathode rays, but to some other force or forces of a much more potent nature.

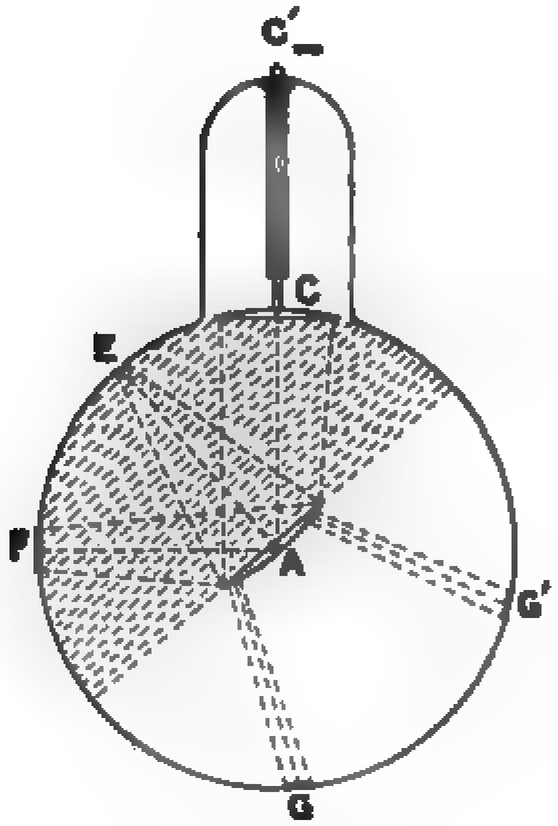
The Mode of Reflection of Cathode Rays.

The reflection of cathode rays is largely diffuse, but does not appear to be altogether so, as the writer has already pointed out,* and since more completely verified by the following further investigations.

In the first place, experiments were made to see whether any direct visual evidence of specular reflection of cathode rays could be obtained with a spherically concave reflector. For this purpose the tube shown in fig. 4 was constructed. In this tube the cathode C is made of aluminium 1·25 inches diameter, and is spherically concave on its active surface with a curvature of a 10-inch sphere, so as to give a fairly parallel beam of cathode rays. The anti-cathode A consists of a circular disc of platinum about 0·9 inch diameter, stamped into a mould having a curvature equal to that of a 10-inch sphere, the concave surface of the platinum being highly polished. The anti-cathode is mounted on a vertical spindle, arranged in guides, and furnished with a small brass bob weight attached to a horizontal arm, so that by tapping the tube the anti-cathode can be rotated round its vertical axis into any desired position. The diameter of the spherical bulb, at the centre of which the anti-cathode is placed, is 3·6 inches, and an additional wire electrode is provided in an annex at the top to serve as anode. In experimenting with this tube, a 10-inch induction coil, with

* 'Roy. Soc. Proc.,' vol. 63, p. 436.

FIG. 4.



mercury contact breaker, working at about one-half full power, was employed, and when this coil was connected through two spark gaps to the cathode C and the spare anode, the anti-cathode A being connected to earth, the following phenomena were observed.

With the anti-cathode so placed, as shown in fig. 4, that the cathode rays impinged on its concave side at an average angle of about 135°, in addition to the slight general green fluorescence of about half the bulb, due to the diffuse reflection of the incident cathode rays by the concave anti-cathode, which fluorescence, as indicated in the illustration, did not differ from what is usually observed in focus tubes, there appeared two very bright and somewhat unstable fluorescent patches upon the glass of the bulb facing the concave side of the anti-cathode. One of these patches, E, which was approximately of circular form, was directly opposite the concave side of the anti-cathode, and was connected to the latter by a faintly luminous beam, while the other, F, which was of a horizontally elongated form, had a position corresponding with the extremity of a second luminous beam apparently of cathode rays reflected from the anti-cathode in true specular fashion. Further, on the glass facing the convex side of the anti-cathode, there at the same time appeared a large-diameter hollow ring of very faint fluorescence, GG'.

On slightly rotating the anti-cathode in either direction, both the *patches and* the ring were also found to move, the circular patch E, *and the ring* GG', maintaining a position respectively exactly in front

of and behind the anti-cathode, while the elongated patch, F, moved to an extent that showed that the angular displacement of the reflected beam of cathode rays that occasioned it was twice the angular displacement of the reflecting surface.

The patch E, and the ring GG′, appear to be due to some description of rays given off directly by the anti-cathode normally to its concave and convex surfaces respectively, and independently of the position of the anti-cathode on its axis. The exact nature of these normal anti-cathode rays at present appears uncertain, and calls for further investigation.

The fluorescent patch F, from the manner in which both its movements and form obey the usual laws of reflection, seems undoubtedly to be due to cathode rays proceeding initially from the cathode C, and reflected specularly by the concave surface of the anti-cathode.

It should be mentioned that though, when obtained, the fluorescent patches described above are most distinct and unmistakable, they are not always obtained very readily. With the tube used, the patches increased in brightness when the spark gap included in the circuit between the coil and the anode of the tube was made fairly large. The patch F was best obtained when the angle between the incident and reflected beams is greater than 90° and less than 180°. This patch could not be obtained satisfactorily when the angle was much less than 90°, possibly owing to the incident beam interfering with the reflected beam. In order to obtain satisfactory patches, it was found that the anti-cathode must not be used as anode, and must be connected to earth; that there must be at least one spark-gap in the circuit, and that the anode must not be connected to earth. The patches are generally somewhat unsteady as regards position, being apparently affected by the varying electrification of the glass walls of the tube.

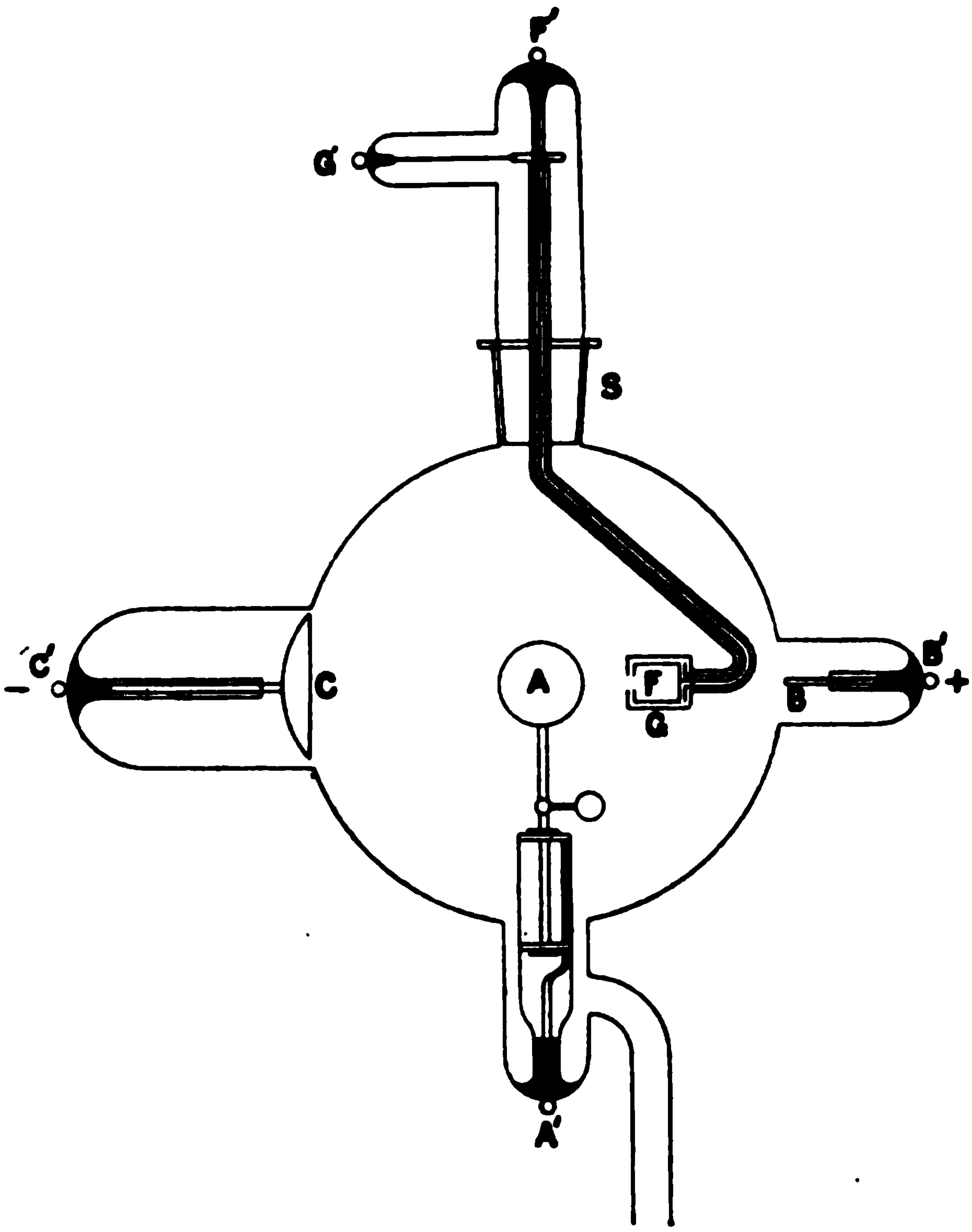
The patches were best obtained with the tube exhausted to about 0·000024 atmosphere, when the general green fluorescence over half the bulb, due to diffusely reflected cathode rays, was but faintly visible. When the degree of exhaustion was raised above that stated, the patches became larger and fainter, and finally disappeared, merging in the general fluorescence.

Quantitative Results.

Endeavours were next made to obtain accurate quantitative measurements of the cathode rays reflected from a flat and highly polished platinum surface by catching a definite portion of the reflected rays in a movable Faraday cylinder connected to earth through a galvanometer, and noting the amount of charge imparted to the cylinder for different angles both of incidence and reflection.

The first tube constructed for this purpose is shown in fig. 5. Here

FIG. 5.



the spherically concave cathode C is 1·25 inch diameter and 0·75 inch radius of curvature. A is the anti-cathode reflector, consisting of a plain disc of polished platinum 0·5 inch diameter, mounted on a vertical axis held in guides, and provided with a small bob weight, so that by tapping the tube, the reflector can be set in the position necessary to give any desired angle between its surface and the incident cathode rays. The electrode B is provided for use as anode. The Faraday cylinder is constructed with an inner and outer cylinder of brass, F and G, similar to those described in connection with fig. 2. The apertures into both cylinders are about 0·08 inch diameter. The cylinder is carried by a curved arm of glass tube, fixed to the glass stopper S, which is very carefully ground into the neck of the tube; a copper wire passing through the tubular arm serves to connect the inner Faraday cylinder with the terminal F′, while a thick coating of copper electrolytically deposited over the entire outside surface of the glass arm, and connected at one end to the outside Faraday cylinder G, and at the other end to the terminal G′, can be earthed and serves to

screen the inner cylinder and its connection from all outside influence. The experiments were conducted with the tube connected to the mercury pump, and the ground-glass stopper S being lubricated with a little vaseline was found to maintain the vacuum very well for considerable periods, while at the same time permitting of easy rotation of the Faraday cylinder round A into any desired position without the vacuum being impaired. A circular scale of cardboard attached to the tube around the neck near S, allowed of the angles made by the surface of A and the axis of the cylinder F with the axis of the cathode stream proceeding from C being accurately determined. The cathode C and the anode B were directly connected, without spark gaps, to a 10-inch Ruhmkorff coil with mercury contact breaker, working at about ¼ full power. The reflector A, as also the terminal G′, were joined up to an earth connection which special tests had shown to be efficient, while the inner Faraday cylinder was connected by means of F′ also to earth through a D'Arsonval mirror galvanometer, having 250 turns of wire in its coil. The tube was connected with a mercury pump, and also with a McLeod gauge. Even after prolonged exhaustion, it was found that much electrical power could not be applied to the tube for any length of time without largely deteriorating the vacuum, but with less power the latter was more constant. Even then the vacuum was found always to be slightly lower at the end of a series of observations than at the beginning, and in order to avoid this disturbing factor, every series was taken first one way and then again in reverse order, the mean of the two sets giving results from which the influence of the gradual decrease in the degree of exhaustion was very nearly eliminated.

Fig. 6.

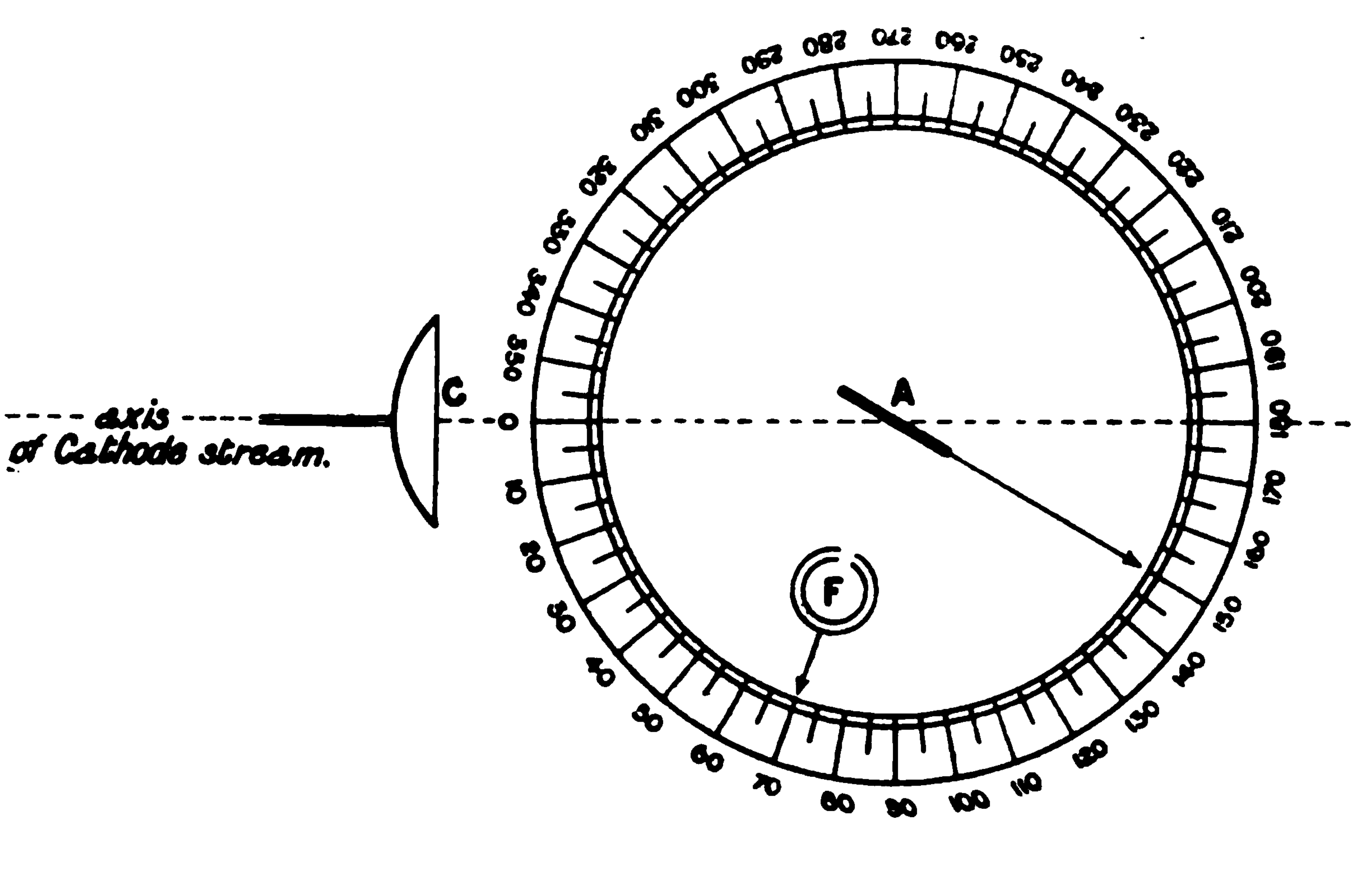


Fig. 6 shows the arrangement of the scale, from which the positions of the reflector and the Faraday cylinder relatively to the axis of the primary cathode stream can be seen for each observation in the following tables. The observed galvanometer deflections are in degrees of an arbitrary scale.

The first experiments were made with the Faraday cylinder stationary, the anti-cathode reflector being rotated; a specimen set of the results obtained is given in Table I.

Table I.

Faraday cylinder fixed at 90°.
Readings taken with reflector at different angles from 75° to 175°.
Pressure 0·000018 to 0·000023 atmosphere.

Reflector.	First series deflection.	Second series deflection.	Mean deflection.
75°	0·25	0·3	0·275
90	0·35	0·5	0·425
100	0·75	1	0·875
110	1·5	1·75	1·625
120	2·2	3	2·6
130	3	3·3	3·15
140	3·3	3·3	3·3
150	3	3·1	3·05
160	2·7	2·6	2·65
170	1·6	2	1·8
175	0·4	0·5	0·45

It will be observed that as the reflector was rotated the galvanometer deflections gradually increased in value up to a certain point, and then decreased again; also that the maximum mean deflection was obtained with the reflector at 140°, *i.e.*, very nearly at that position which would give equal angles of incidence and reflection for the cathode rays.

Next, the reflector was kept stationary, and the cylinder moved so as to explore the field of reflected rays. A specimen set of the results thus obtained is given in Table II.

In this instance, on the assumption of partial specular reflection, the maximum galvanometer deflection should of course be obtained with the cylinder as near to 0° as it could be placed without interfering with the primary cathode rays. It was not found practicable to place it nearer than 45°, but, as will be observed, the deflections rise steadily up to this latter position.

Several other series of observations were made with this tube, with the cylinder stationary at 45° and at 130°, with the reflector at varying angles, and also with the reflector stationary at 70° and at 135°, and

Table II.

Reflector fixed at 90°.

Readings taken with the Faraday cylinder at different positions from 95° to 45°.

Pressure 0·00001 to 0·000015 atmosphere.

Cylinder.	First series deflection.	Second series deflection.	Mean deflection.
95°	0·3	0·2	0·25
85	0·6	0·5	0·55
75	1	1·1	1·05
65	1·1	1·4	1·25
55	1·4	1·6	1·5
45	1·7	1·7	1·7

with the cylinder in various positions. In all instances a maximum deflection was obtained with positions that made the angles of incidence and reflection approximately equal, and smaller and smaller deflections resulted the further this position was departed from. However, as more accurate readings were afterwards obtained with another tube and more delicate arrangements, described below, it has been thought best to omit detailed particulars of these observations.

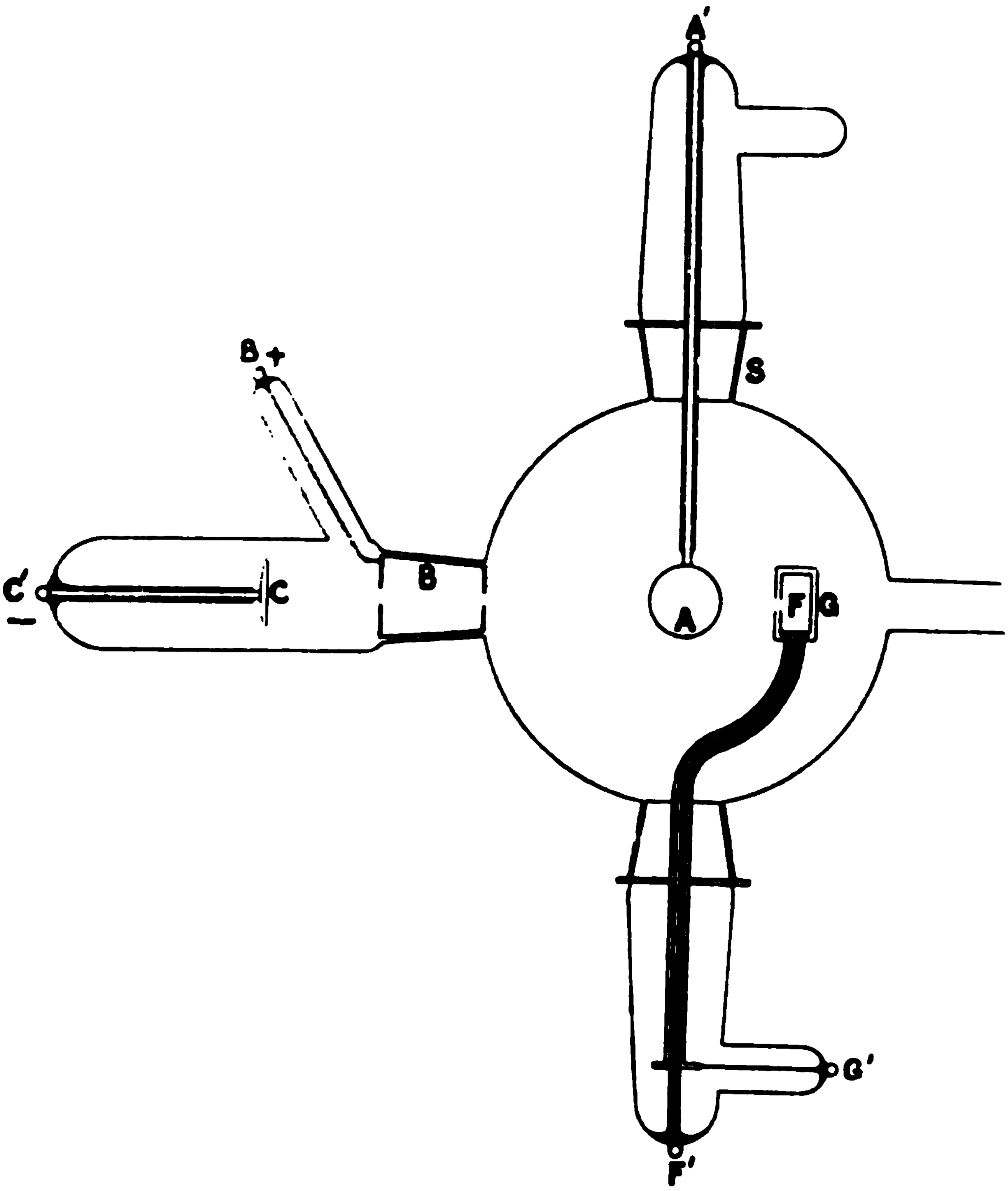
The above have, however, been mentioned to show that two tubes of different descriptions gave similar results, and also that the galvanometer method of measuring the charge imparted to the Faraday cylinder, as described above, gave similar results as the quadrant electrometer used in the further experiments.

The new tube with which experiments were next proceeded with is shown in fig. 7. The arrangements for rotating the reflector in the previous tube having been found somewhat unsatisfactory, in the new tube both the reflector and the Faraday cylinder were attached to ground stoppers, and furnished with pointers and scales, as in fig. 6, so that the angles could be adjusted with great nicety. Further, in order to obtain greater parallelism in the primary cathode stream, an arrangement of cathode and anode was adopted similar to that used by Professor J. J. Thomson.* According to this arrangement the cathode rays from C are directed through two apertures about 0·1 inch diameter in the hollow brass cylinder B, which is ground into a glass neck, and by means of the terminal B′ is used as anode.

It being found that the employment of little electric power was conducive to the maintenance of a constant vacuum, a 6-inch Ruhmkorff coil was substituted for the larger one used previously, and as with the diminished power, and with the very attenuated beam of cathode rays

* 'The Discharge of Electricity through Gases,' by J. J. Thomson, pp. 152 and 164.

FIG. 7.



that could pass through the small apertures in B, the D'Arsonval galvanometer would not give satisfactory readings even when a coil with 500 turns was used, a reflecting quadrant electrometer was employed instead. The needle of this electrometer was connected to the inner Faraday cylinder F by means of an insulated wire threaded through a lead pipe connected to earth so as to exclude all outside influence, and the electrometer was otherwise connected up according to Mascart's method. The deposition of moisture upon the tube being found to affect the results, incandescent electric lamps were arranged so as to keep the whole at a uniform temperature slightly above that of the surrounding atmosphere. In the subsequent experiments the anode B was always connected to earth.

In the following tables each unit of the number given as the deflection of the electrometer corresponds to a mean pressure of about 3 volts, though very probably, owing to the intermittent nature of the discharge, the actual instantaneous value was much higher. In each case the readings of the second series of observations were taken in

reverse order to those of the first series, and P, the pressure given in millionths of an atmosphere, was obtained by the McLeod gauge at the commencement and end of each series. Deflections indicating a positive charge given to the Faraday cylinder are so marked; all others denote negative charges.

Table III.

Faraday cylinder fixed at 45°.
Readings taken with Reflector at different angles from 45° to 180°.

Reflector.	First series deflection.	Second series deflection.	Mean deflection.
	P = 41	P = 54	
45°	0	0	0
50	0	0·5	0·25
55	2	1	1·5
60	2	1·5	1·75
65	4·5	3	3·75
70	8·5	7	7·75
75	15	13	14
80	20·5	18	19·25
85	24	21	22·5
90	25	25	25
95	27·5	27	27·25
100	29	30	29·5
105	29·5	32	30·75
110	30·5	33	31·75
115	31	34	32·5
120	31	34·5	32·75
125	30	33·5	31·75
130	28·5	34·5	31·5
135	28	33	30·5
140	26	31	28·5
145	23	30	26·5
150	21·5	26·5	24
155	18	25·5	21·75
160	14·5	20	17·25
165	10	16	13
170	6	6·5	6·25
175	4	1·5	2·75
180	0·5	0	0·25
	P = 48	P = 45	

Table IV.

Faraday cylinder fixed at 90°.
Readings taken with Reflector at different angles from 90° to 180°.

Reflector.	First series deflection.	Second series deflection.	Mean deflection.
	P = 52	P = 53	
90°	+0·5	0	+0·25
95	0	0	0
100	0·5	0·5	0·5
105	1	1	1
110	3·5	2·5	3
115	7	3·5	5·25
120	11	7	9
125	18·5	10	14·25
130	21·5	12	16·75
135	23·5	14	18·75
140	24·5	14·5	19·5
145	24	14	19
150	22	13·5	17·75
155	18·5	13	15·75
160	15	12	13·5
165	11	11·5	11·25
170	5	6·5	5·75
175	3	2·5	2·75
180	0·5	0·5	0·5
	P = 44	P = 44	

Table V.

Faraday cylinder fixed at 112·5°.
Readings taken with Reflector at different angles from 112·5° to 180°.

Reflector.	First series deflection.	Second series deflection.	Mean deflection.
	P = 46	P = 41	
112·5°	+0·5	+1	+0·75
115	+0·5	+0·5	+0·5
120	0	0	0
125	1	3	2
130	6	16	11
135	18·5	26·5	22·5
140	27·5	45	36·25
145	29	51	40
150	32	54	43
155	31	52	41·5
160	27·5	47	37·25
165	20	39	29·5
170	11	11	11
175	2	0	1
180	0	+0·5	+0·25
	P = 48	P = 32	

Table VI.

Faraday cylinder fixed at 135°.
Readings taken with Reflector at different angles from 135° to 180°.

Reflector.	First series deflection.	Second series deflection.	Mean deflection.
	P = 54	P = 50	
135°	3	2	2·5
140	2	1	1·5
145	26	10	18
150	28·5	62	45·25
155	50	64	57
160	46	64	55
165	44	66	55
170	30	70	50
175	29	19	24
180	27	9	18
	P = 62	P = 47	

Table VII.

Reflector stationary at 67·5°.
Readings taken with Faraday cylinder at different positions from 67·5° to 247·5°.

Cylinder.	First series deflection.	Second series deflection.	Mean deflection.
	P = 40	P = 55	
67·5°	+1	+0·5	+0·75
60	+0·5	0	+0·25
50	2	1	1·5
40	9	5·5	7·25
30	17·5	10	13·75
20	22	12	17
10, 0, 350	Cylinder interfering with primary cathode rays.		
340	37	24·5	30·75
330	42	28	35
320	44	30	37
310	44	31	37·5
300	40·5	31	35·75
290	33	28	30·5
280	24	22	23
270	15	10	12·5
260	2	0·5	1·25
250	0	+0·5	+0·25
247·5	+0·5	+0·5	+0·5
	P = 47	P = 47	

Table VIII.

Reflector stationary at 90°.
Readings taken with Faraday cylinder at different positions from 90° to 270°.

Cylinder.	First series deflection.	Second series deflection.	Mean deflection.
	P = 46	P = 55	
90°	0	0	0
80	0·5	0·5	0·5
70	3	1·5	2·25
60	19·5	4·5	12
50	26	10	18
40	32	13	22·5
30	32	14·5	23·25
20, 10, 0, 350, 340	Cylinder interfering with primary cathode rays.		
330	28	21	24·5
320	26	20·5	23·25
310	22	19	20·5
300	16	20	18
290	8	10	9
280	1	1	1
270	0	0	0
	P = 49	P = 47	

Table IX.

Reflector stationary at 135°.
Readings taken with Faraday cylinder at different positions from 135° to 15°.

Cylinder.	First series deflection.	Second series deflection.	Mean deflection.
	P = 43	P = 51	
135°	+1	0	+0·5
125	4	2	3
115	20	22	21
105	33	31	32
95	38·5	38	38·25
85	55·5	41	48·25
75	40	43·5	41·75
65	37·5	43·5	40·5
55	37·5	43	40·25
45	35	41·5	38·25
35	32	39	35·5
25	29·5	35	32·25
15	22	24·5	23·25
	P = 53	P = 47	

The above are a few typical examples of a much larger number of sets of observations, all giving similar results. On examination it will be seen that in all, both in the cases when the reflector was moved so as to reflect the cathode rays at different angles into the stationary Faraday cylinder, and also when the reflector was stationary and the field of reflected rays explored by moving the cylinder, the effects are approximately similar. In each case the electric charge imparted to the cylinder, as measured by the electrometer deflection, is greatest for almost exactly those positions of reflector and cylinder relatively to the primary cathode rays that would make the angle of reflection most nearly equal to the angle of incidence, the electrometer deflections diminishing gradually, though not at a uniform rate, the greater the departure from this condition. Any slight discrepancies are readily accounted for by the difficulties of maintaining a constant vacuum and uniform action of the induction coil contact breaker, and are also possibly, in some instances, due to electrostatic repulsion experienced by the reflected cathode rays. It would, therefore, appear that the reflection of cathode rays by a flat polished platinum surface is not altogether diffuse, but takes place to some considerable extent in a more or less specular manner.

As will be observed in several of the sets of observations, a small reverse deflection of the electrometer, indicating a slight positive charge of the cylinder, was obtained either at the end or beginning of a series, when the relative positions of reflector, cylinder, and primary cathode rays would allow of no reflected cathode rays entering the cylinder. This curious fact requires further investigation.

In order to ascertain whether the intensity of the reflected cathode rays would increase as the incidence was made more slanting, several series of observations were made, where both the reflector and cylinder were moved, the latter at twice the rate of the former, in such a manner as to measure the maximum intensity of the reflected rays for varying angles of incidence. The following table (Table X) gives the mean deflections obtained with four series, which appear to show that the intensity of the reflected rays does increase as the incidence is more slanting. The increase in the early stages is not, however, great; while it is possible that in the latter stages some direct cathode rays obtained access to the cylinder.

Charge Imparted to the Reflector.

Experiments were also made to ascertain whether the charge imparted to the reflector varied with the angle of incidence of the primary cathode rays. The results are given in Table XI, from which it will be seen that the electrification of the reflector while strongly negative for normal incidence of the cathode rays, becomes zero at an angle between 130° and 135°, and slightly and increasingly positive for still larger angles. Comparing this result with that obtained in the pre-

Table X.

Both Reflector and Cylinder moved, the latter at twice the angular rate of the former.

Reflector.	Cylinder.	Mean deflection.
100°	20	25·875
105	30	28·375
110	40	29·375
115	50	29·5
120	60	29·75
125	70	29·75
130	80	30·25
135	90	30·875
140	100	32·25
145	110	36·75
150	120	43·25
155	130	52·125
160	140	64·375
165	150	79·125

ceding experiment given in Table X, it will be noted that as the negative charge imparted to the reflector diminishes the maximum charge conveyed by the reflected cathode rays increases, though the rates of diminution and increase respectively are by no means equal.

Table XI.

Reflector connected to electrometer.

Reflector.	First series deflection.	Second series deflection.	Mean deflection.
90°	76	35	55·5
95	72	33·5	52·75
100	71	31	51
105	63	27	45
110	50	24	37
115	37	15	26
120	23	11·5	17·25
125	13	7	10
130	4	4	4
135	+1	0·5	+0·25
140	+2·5	0	+1·25
145	+3	0	+1·5
150	+3	0	+1·5
155	+3	0	+1·5
160	+3·5	0	+1·75
165	+4	+0·5	+2·25
170	+5	+1	+3

The writer has previously described* how with an anticathode, inclined at an angle of 45° to the axis of a conical cathode stream, he found, by examination with a pin-hole camera, that those portions of the stream which impinge most normally upon the anticathode are the most efficient in producing Röntgen rays, while those portions of the stream which strike the anticathode surface very much on the slant are less efficient in producing Röntgen rays. There is probably some connection between this and what is indicated in Tables X and XI. The fact that the more normal is the angle of incidence, the greater is the amount of negative charge imparted to the anticathode reflector, the greater the amount of Röntgen rays produced, and the less the amount of charge in the reflected cathode rays, would seem to support the view that the Röntgen rays are actually generated in some way by the electric charges carried by the cathode ray particles being imparted to the anticathode.

Conclusion.

The results of the experiments described above differ in at least one important particular from those obtained by Mr. H. Starke, an account of whose researches appeared in Wiedemann's 'Annalen,' No. 9, p. 56, 1898, while the writer's investigatons were in progress. Mr. Starke, using a form of tube in which the arrangement of cathode, anode, and reflector was very similar to that shown in fig. 9, but with a Faraday cylinder fixed in one definite position, as in the tube illustrated in fig. 2, and using the galvanometer method of measuring the charge conveyed to the cylinder by the reflected cathode rays, appears to have found that so long as the same face of the reflector was turned towards both the cathode and cylinder, the orientation of the reflector did not affect the amount of charge conveyed to the cylinder. This is so totally at variance with the results given above, which were repeated over and over again, that the writer can only assume that the methods employed by Mr. Starke were not as sensitive as his own, particularly as in the case of the writer's results those obtained by rotating the reflector, with the cylinder stationary, are confirmed by those obtained with a stationary reflector and a movable cylinder—the latter method not having been employed by Mr. Starke.

In conclusion, the writer desires to express his great indebtedness to the valuable assistance of Mr. J. C. M. Stanton and Mr. H. L. Tyson Wolff in carrying out the above investigations.

* 'Roy. Soc. Proc.,' vol. 63, pp. 434–5.

"On the Order of Appearance of Chemical Substances at different Stellar Temperatures." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received February 17,—Read February 23, 1899.

In a paper on the "Chemistry of the Hottest Stars,"* in 1897, I stated the results so far arrived at concerning the order in which certain spectral lines appeared and others disappeared in stars arranged in a series of ascending temperatures. Since that paper was written many important advances have been made, so that I have been able in the meantime to considerably extend the research. Among these advances I may mention the following:—

1. With regard to the metals, my recent note on the enhanced lines in the spectrum of α Cygni† enables us to deal with the lines observed at the highest temperature in the spectra of the following substances, Fe, Mg, Ca, Sr, V, Ti, Ni, Mn, Cr, and Cu.

The temperature ranges of the enhanced lines of these metals have been investigated with the following results:—

Metal.	Range of temperature (upward series).	Range of temperature (downward series).
Mg	α Ursæ Min. to γ Argûs	α Eridani to Procyon.
Ca	α Tauri to γ Argûs	α Eridani to Arcturus.
Fe	α Tauri to ζ Tauri	β Persei to Arcturus.
Ti	α Tauri to ζ Tauri	β Persei to Arcturus.
Cu	α Ursæ Min. to α Cygni	β Persei to Procyon.
Mn	α Ursæ Min. to α Cygni	β Persei to Procyon.
Ni	α Ursæ Min. to α Cygni	β Persei to Procyon.
Cr	α Ursæ Min. to α Cygni	γ Lyræ to Procyon.
V	α Ursæ Min. to α Cygni	Sirius to Procyon.
Sr	α Tauri to α Cygni	Sirius to Arcturus.

I pointed out in the note referred to that the enhanced lines of the above substances seemed to account for almost all of the more marked lines in α Cygni. It is on this ground that I have investigated their behaviour in other stars before waiting for the results of the complete inquiry. Another reason has been that although in addition to the enhanced lines of the metals shown in the foregoing table, those of

Ba, Cd, Mo, La, Sb, Pd, Ta, Rh, Er and Yt, Ce, Wo, U, Zr, Pb, Co, Bi,

have already been investigated with lower dispersion, and a spark obtained with the use of a much less jar capacity, so far I have no

* 'Roy. Soc. Proc.,' vol. 61, p. 148.

† *Supra*, p. 320.

certainty that any of these substances exist in the reversing layers of stars of intermediate temperature.

2. The temperature ranges of the arc lines of some of the metals have also been investigated, and the results are shown in the following table:—

Metal.	Range of temperature (upward series).	Range of temperature (downward series).
Fe	α Tauri to α Cygni	α Canis Majoris to Arcturus.
Ca	α Tauri to α Ursæ Min.	α Canis Majoris to Arcturus.
Mn	α Tauri to α Ursæ Min.	α Canis Majoris to Arcturus.

3. The new series of lines discovered by Professor Pickering, and described by him as representing a new form of hydrogen,* has been found in the spectra of ζ, ε, δ, and κ Orionis photographed at Kensington in 1892, and Mr. McClean has traced the lines in γ Argûs.

We are therefore now in a better position to determine the relation of this new gas to other gases, both known and unknown, appearing in stars of nearly equal temperature.

4. In addition to the unknown lines at λλ 4089·2 and 4649·2, referred to in my last communication on this subject,† three other unknown lines occur in γ Argûs.

As these most probably reveal still undiscovered gases, I include them in the following table, showing the limits of stellar temperature to which the various known and unknown lines, probably of gaseous origins, extend.

Origin.	λ of chief lines.	Range in ascending series of stars.	Range in descending series of stars.
Unknown . .	4457, 4451, 3876	Seen only	in γ Argûs.
Hydrogen (new)	4544·0, 4200·4	ζ Orionis to γ Argûs	No stars available.
Unknown . .	4089·2	α Crucis to ζ Orionis	
"	4649·2	" "	α Eridani.
Helium . .	4471·6, 4026·3	Rigel to γ Argûs	α Eridani to γ Lyræ.
Asterium . .	4388, 4009	" "	α Eridani to γ Lyræ.
Hydrogen . .	complete series	Aldebaran to γ Argûs	α Eridani to Arcturus.

* 'Astrophys. Journ.,' vol. 5, p. 92 (1897).

† 'Roy. Soc. Proc.,' vol. 62, p. 52.

5. Mr. McClean has stated that certain of the oxygen lines (amongst which is the strong triplet at λλ 4070·1, 4072·4, and 4076·3) appear in the spectrum of β Crucis and other stars of nearly equal temperature. My own observations so far as they have gone tend to confirm this view, but other photographs and more laboratory work are needed to explain certain changes of intensity which have been observed. The lines attributed by Mr. McClean to oxygen have been noted between α Crucis and ζ Orionis in the upward series, and in stars at about the α Eridani stage of temperature in the downward series.

6. There is evidence that the strongest lines of nitrogen at λ 3995·2 and λ 4630·9 make their appearance in stars at about the temperature of α Crucis. These lines appear from Rigel to ζ Orionis in the upward series, and are present in stars at the α Eridani stage in the downward.

7. I pointed out many years ago* that at high temperatures, the flutings of carbon in the violet are replaced by a line at λ 4267·5. There is a line at this wave-length in the spectra of stars ranging in temperature from that of Rigel to ζ Orionis on the up side, and from α Eridani to β Persei on the down side, of the temperature curve.

There is no known line of gases or metals to which this line can be assigned. It is probable therefore that carbon exists in stars of the same temperature as that at which oxygen and nitrogen have been traced.

8. Two lines in the spectrum of silicium (λ 4128·5 and λ 4131·5) have been traced in stars between the temperatures of α Ursæ Min. and α Crucis in the upward series and between those of α Eridani and Procyon in the downward.

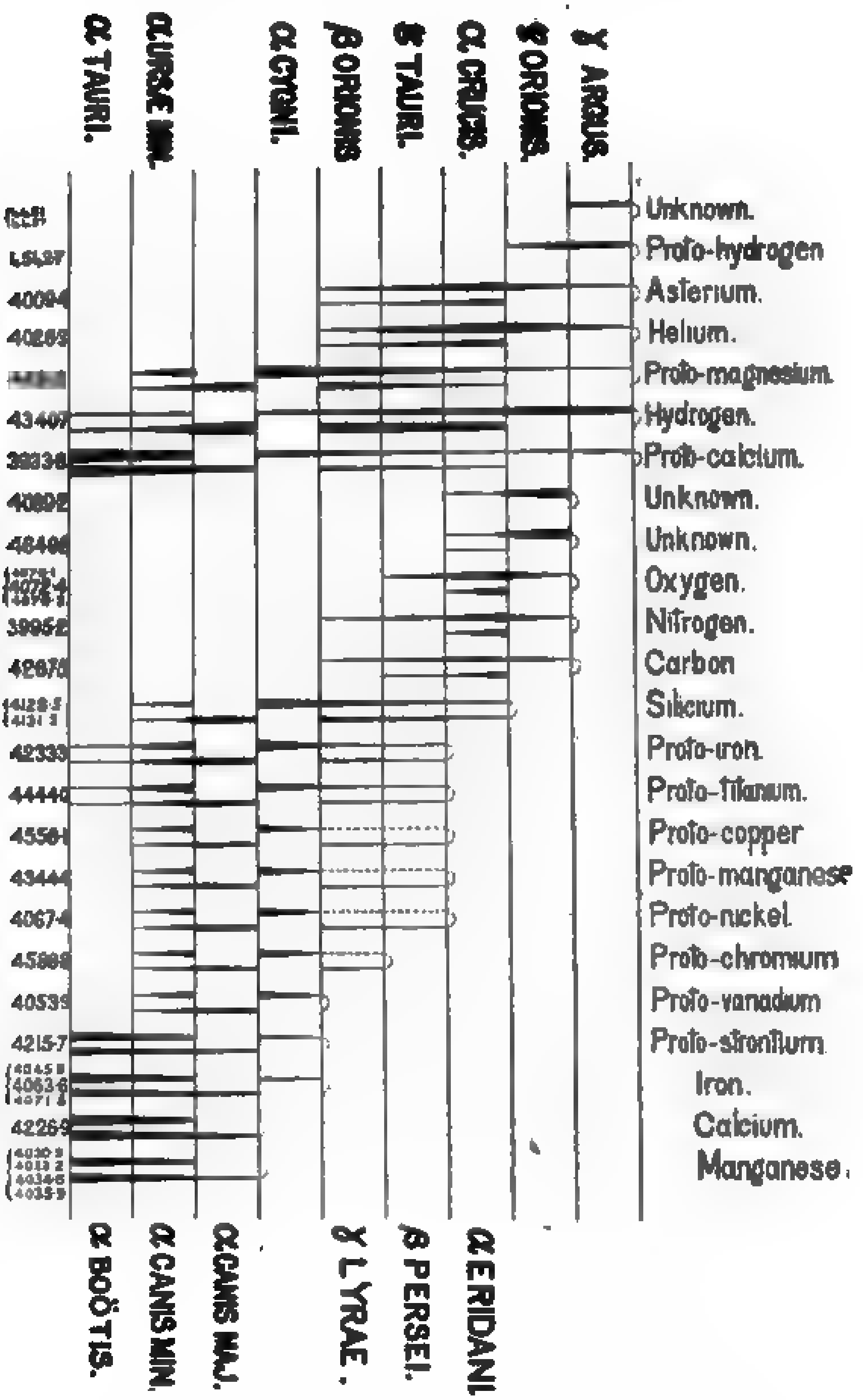
The accompanying map shows the facts relating to stars as hot as, or hotter than, the sun, as we know them at present.

Description of Map.

The map is arranged on the following plan: The temperature of the sun and Arcturus forms the lowest stage. The upper limit is defined by γ Argûs, the hottest star so far known. On the left the stars named are those of increasing temperature; on the right those of decreasing temperature. Those on the same horizon represent equal mean temperatures so far as the cleveite gas and enhanced lines help us to determine them. The blank spaces indicate that so far no star has been photographed in the spectrum of which the enhanced lines exactly match those on the opposite side.

The names of the various chemical substances included in the discussion are given at the top. I have retained the prefix "proto-" to that condition of each metallic vapour which gives us the enhanced lines alone, and I have added it to that form of hydrogen seen only in the hottest stars.

* 'Roy. Soc. Proc.,' vol. 30, p. 461.



The behaviour of the most typical line of each chemical substance is indicated by a double line looped at the top at its highest range. The length and varying thickness of the lines in stars on both sides of the temperature curve are derived from the observed appearance and *intensity of the lines*, noted in the different stars.

The wave-lengths of the lines discussed are shown at the bottom of the map.

ADDENDUM.

The facts embodied in the map present to us the spectral changes noted in stars of Groups III, IV, and V, of my classification,* and are a result of a more general inquiry than those referred to in my previous papers,† the origins of a very considerable number of stellar lines having since then been traced to enhanced lines of metals and to known gases.

It will be seen that this more general inquiry entirely justifies the prior statement‡ that the metallic lines are thickest in stars increasing their temperatures, and that the hydrogen lines are thickest in stars decreasing their temperatures; in other words, on the opposite arms of the temperature curve. I have already stated a possible explanation.§

It will be observed that, so far, I have not been able to find stellar spectra on the downward side corresponding to those of γ Argûs and ζ Orionis; but it is more than probable that near the apex of the curve only a small change will be observed; their default, therefore, is of less consequence than it might have been.

The same remark applies to α Cygni and Sirius; but here it is certain that the differences in the relative intensities of the gaseous and enhanced lines will be considerable, judging from what happens above and below the heat stages represented by them.

The stars used in the discussion give us very definite results, showing that the various chemical forms are introduced at six very distinct heat levels.

I next proceed to make some remarks upon the series of facts now for the first time brought together; it must, however, be borne in mind that all the chemical elements and all parts of the spectrum have not yet been included in the survey.

1. Hydrogen appears throughout both series of stars from top to bottom. Proto-magnesium and proto-calcium follow suit very nearly; but the highest intensity of the former is reached at the stage represented by α Cygni, and of the latter at the solar temperature represented by α Tauri and Arcturus.
2. With the above exceptions, all the chemical forms so far traced are relatively short-lived.

This is the first important differentiation. In the light of (1) we

* 'Roy. Soc. Proc.,' vol. 43, p. 117 (1887).

† 'Roy. Soc. Proc.,' vol. 44, p. 1 (1888); 'Roy. Soc. Proc.,' vol. 45, p. 380 (1889); 'Phil. Trans.,' A, vol. 184 (1893), p. 725.

‡ 'Roy. Soc. Proc.,' vol. 61, p. 182.

§ 'Roy. Soc. Proc.,' vol. 61, p. 183.

are justified in assuming that the substances in (2) would be visible in the stellar reversing layers if they were there.

3. In the stars of higher temperatures we deal generally with gases. Below the stages represented by β Orionis and γ Lyræ we deal with proto-metals and metals, hydrogen being the only exception.
4. The proto-metals make their appearance at about the same heat-level at which the gases (with carbon), always excepting hydrogen, begin to die out.

This is the second important differentiation. It is interesting to notice the distinct difference of behaviour of carbon and silicium in the descending series; the former goes through the same stages as oxygen and nitrogen, the latter behaves like the proto-metals.

5. With the exception of iron the metals, as contradistinguished from the proto-metals, only make their appearance in stars at and below the heat-level of Sirius.

This is the third important differentiation. It is accompanied with a notable *diminution* of hydrogen and proto-magnesium, and with an *increase* of proto-calcium: indeed the latter seems generally to vary inversely with the hydrogen.

In all these changes we seem to be brought into presence of successive polymerisations due to reduction of temperature. Of the origin of proto-magnesium and proto-calcium the stars as yet tell us nothing, but it is difficult to believe that the earliest forms of the other metals are not built up of some of the constituents of the heat ranges represented by those between γ Argûs and α Crucis.

The question arises whether the order of visibility at reduced temperatures now indicated does not explain the absence of proto-hydrogen, oxygen, and nitrogen from the spectra of the sun and nebulæ, the metals present in, and the absence of quartz from, meteorites, and the similarity of the gaseous products obtained from them and metals, native and other, *in vacuo* at high temperatures.

I have finally to express my obligations to those who have aided me in the present inquiry. For some of the metals used I am indebted to Mr. George Matthey, F.R.S., who has kindly placed the resources of his establishment so entirely at my disposal that I feel it is impossible to thank him sufficiently. For the determination of wave-lengths and the correspondence of terrestrial and stellar lines, Mr. Baxandall is responsible, while Mr. Fowler has assisted in the determination of the various stellar groups. The photographs of the enhanced lines obtained by the use of the Spottiswoode coil have been taken by Mr. Butler. The actual construction of the map from the available photographs has devolved upon Mr. Baxandall, Dr. Lockyer co-operating in the case of stars of the highest temperature.

March 2, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes the names of Candidates for election into the Society were read as follows:—

Adeney, Walter Ernest, D.Sc.
Allen, Alfred Henry, F.C.S.
Ardagh, Sir John, Major-General, R.E.
Ballance, Charles Alfred, F.R.C.S.
Barrett, Professor W. F., F.R.S.E.
Booth, Charles.
Bridge, Professor Thomas William, M.A.
Brown, John.
Bruce, Surgeon-Major David, M.B.
Budge, Ernest A. Wallis, D.Litt.
Callaway, Charles, D.Sc.
Cardew, Philip, Major, R.E.
Copeman, Sydney Monckton, M.D.
Crookshank, Professor Edgar March, M.B.
Darwin, Horace, M.A.
David, Professor T. W. Edgeworth, B.A.
Dixon, Professor Alfred Cardew, M.A.
Dixon, Professor Augustus Edward, F.C.S.
Feilden, Colonel Henry Wemyss.
Fenton, Henry John H., M.A.
Gamble, James Sykes, M.A.
Gray, Professor Thomas, B.Sc.
Haddon, Professor Alfred Cort, M.A.
Hamilton, Professor David James, M.D.
Harmer, Frederic William, F.G.S.
Head, Henry, M.D.
Hiern, William Philip, M.A.
Hill, Leonard, M.B.
Hills, Edmond Herbert, Captain, R.E.
Hopkinson, Edward, M.A.
Jackson, Henry Bradwardine, Captain, R.N.
Lansdell, Rev. Henry, D.D.
Lister, Joseph Jackson, M.A.
MacArthur, John Stewart, F.C.S.
MacGregor, Professor James Gordon, D.Sc.
Maclean, Magnus, D.Sc.
Mallock, Henry Reginald Arnulph.
Mance, Sir Henry C., C.I.E.
Mansergh, James, M.Inst.C.E.
Marsh, James Ernest, M.A.
Mather, Thomas.
Matthey, Edward, F.C.S.
Mill, Hugh Robert, D.Sc.
Morgan, Professor Conwy Lloyd, F.G.S.
Muir, Thomas, M.A.
Notter, James Lane, Surgeon-Lieut.-Col.
Perkin, Arthur George.
Rambaut, Professor Arthur A., M.A.
Reid, Clement, F.G.S.
Russell, James Samuel Risien, M.D.

Salomons, Sir David, M.A.
Saunders, Edward.
Schlich, Professor William, C.I.E.
Sell, William James, M.A.
Shaw, Professor Henry S. Hele, M.Inst.C.E.
Sidgreaves, Rev. Walter, S.J.
Smith, James Lorrain, M.D.
Smith, Professor William Robert, M.D.
Smithells, Professor Arthur, B.Sc.
Spencer, Professor W. Baldwin, B.A.
Starling, Ernest Henry, M.D.
Swinton, Alan Archibald Campbell, Assoc. M.Inst.C.E.
Tanner, Professor Henry William Lloyd, M.A.
Tatham, John F. W., F.R.C.P.
Thomas, Michael Rogers Oldfield, F.Z.S.
Threlfall, Professor Richard.
Tutton, Alfred E., B.Sc.
Ulrich, Professor George Henry Frederic, F.G.S.
Walker, James, M.A.
Walker, Professor James, D.Sc.
Watson, William, B.Sc.
Whitehead, Charles, F.L.S.
Whymper, Edward, F.R.G.S.
Windle, Bertram Coghill Allen, M.D.
Woodward, Arthur Smith, F.G.S.
Wright, Professor Edward Perceval, M.A.

The following Papers were read:—

I. "Perturbations of the Leonids." By Dr. G. J. STONEY, F.R.S. and Dr. DOWNING, F.R.S.

II. "On Flapping Flight of Aëroplanes." By Professor M. F. FITZGERALD. Communicated by Professor G. F. FITZGERALD, F.R.S.

III. "On Hydrogen Peroxide as the Active Agent in producing Pictures on a Photographic Plate in the Dark." By Dr. J. W. RUSSELL, F.R.S.

"Perturbations of the Leonids." By G. JOHNSTONE STONEY, M.A., D.Sc., F.R.S., and A. M. W. DOWNING, M.A., D.Sc., F.R.S. Received February 8,—Read March 2, 1899.

When the present investigation was undertaken, our knowledge of the perturbations of the Leonids was due to an investigation carried on thirty years ago by Professor J. C. Adams.*

His object was to compute the shift in the nodes of the meteoric orbit due to perturbations, and to compare the calculated amount with the amount which had been deduced by Professor Hubert A. Newton from observations made at intervals during the last 1000 years.†

For Professor Adams's purpose the perturbations to be computed

* 'Comptes Rendus,' March 25, 1867, p. 651; and for a fuller account see 'Monthly Notices of the Roy. Astron. Soc.,' April, 1867, p. 247; or 'Monthly Notices,' March, 1897, p. 387, where the last-mentioned paper is reprinted.

† '*Silliman's Journal*,' 1864, vol. 37, p. 377; and vol. 38, p. 53.

were the average perturbations; and he accordingly employed Gauss's method, in which the mass of the disturbing planet is supposed to be distributed round its orbit in quantities proportional to the time that the planet occupies in travelling over each portion of its course. This elegant method furnishes the average amount of each perturbation on the supposition that the periodic times of the disturbed body and of the disturbing planet are incommensurable, so that in the course of time the two bodies present themselves in every possible position to one another.

This condition, however, has been but imperfectly fulfilled within the limited period of 1000 years over which the recorded observations extend, especially in the case of the three planets which influence the Leonids most, and indeed are almost the only planets whose attraction needs to be taken into account. These are Jupiter, Saturn, and Uranus. A comparison of the periodic times shows that fourteen revolutions of Jupiter approximate in duration within about one-fifth of a year, to five revolutions of the meteors; two revolutions of Uranus occupy about one and three-quarters of a year more than this same time, and nine revolutions of Saturn correspond within a fraction of a year to eight revolutions of the meteors.

These cycles have been several times repeated within the period over which the observations extend; and one consequence of these cycles is that there have been oscillations in the rate of the advance of the node about its mean value, so that the times for the showers assigned by applying to the orbit the average shift of its node, have usually differed by several hours from the actual times. On one occasion—in A.D. 1533—the shower anticipated the computed time by about twenty-six hours, and, as the present investigation shows, a deviation of comparable amount and in the opposite direction is to be expected this year. Accordingly, even if our sole object were to enable astronomers in future to predict more satisfactorily the times of the greater Leonid showers, it would be necessary to prepare for the task by first studying the actual amount of the perturbations in each revolution, and moreover, for meteors occupying various stations along the stream.

For, in fact, the perturbations have not only differed in different revolutions, but even within a single revolution, the meteors which occupy successive positions in the procession are differently affected by the surrounding planets, as is confirmed by the definite results which Herr Berberich has obtained by assuming successively two epochs for the perihelion passage.* The dense part of the stream, with which we are chiefly concerned, and which we may call the ortho-stream,† is now so

* See his paper on the perturbations since 1890 of the orbit of the comet which is associated with the Leonids, 'Astr. Nach.,' No. 3526.

† In order to facilitate the study of the Leonids it is convenient to distinguish

long that it takes between two and three years to pass each point in its orbit, so that the configurations in which the several parts are presented to the disturbing planets are markedly different. Accordingly, perturbations must have produced in this long stream both sinuosities and an unequal distribution of density;* and the first step towards increasing our acquaintance with these and other kindred phenomena, as well as towards gaining a better insight into the past history of the swarm, is to aim first at securing a more intimate knowledge of the perturbations.

With this end in view it was decided, as a first step, to compute the actual perturbations of a definite part of the stream over the whole of one revolution, taking that part of the ortho-stream of which Adams had determined the orbit, and extending the computation over the revolution from the date of the great shower of November, 1866, until that day in January, 1900, when the same part of the stream will return to the earth's orbit.

Adams's calculation was based on determinations of the radiant point which were made in 1866, before photography had lent the aid to astronomy which it now yields. Moreover, the circumstance that the earth deflected the meteors which were then observed by an amount which varied as the shower progressed, was not at that time attended to by observers. Owing to these imperfections, there is a considerable probable error in the mean of the determinations which were made in 1866, and a corresponding uncertainty in the values of the elements computed from that mean. We are accordingly only justified in employing Adams's orbit as approximate. But, fortunately, an error in the orbit, of such an amount as is at all likely to exist, will not materially affect the perturbations of the orbit, which are what we have at present in view.

The main stream of Leonids—the ortho-stream—is narrow and very long, and it is convenient to divide it into segments, each of which

between a great body of them—the ortho-Leonids—which are travelling round the sun in nearly identical orbits, and another class of Leonids which we may call clino-Leonids, that are pursuing courses which differ in a more considerable degree from the ortho-orbit. By the ortho-orbit is to be understood the mean of the orbits of the ortho-Leonids.

The ortho-Leonids at present form a compact stream of such a length that it takes nearly three years to pass each point of its orbit, and so narrow that when the earth passes obliquely through it the transit occupies only some five or six hours; whereas the clino-Leonids form a less dense and wider stream, which has spread itself the whole way round the ring, and which produces in every November, when the earth passes through it, a feeble meteoric shower that lasts for several days.

* One consequence of the existence of irregularities in the stream of ortho-Leonids is that the ortho-orbit at one cross-section of the stream (*i.e.*, the mean of the orbits of the meteors occupying that situation in the stream) is in general not absolutely identical with the ortho-orbits at other cross-sections.

shall be of moderate length. Through one of these, which we may call segment A, the earth passed in November, 1866, and on that occasion there was withdrawn from it that small portion which consisted of meteors which either encountered or passed close to the earth. Those that actually plunged into the earth's atmosphere were destroyed: those that passed near were deflected, and were also either accelerated or retarded, and they thus became clino-Leonids. It is with the great majority of the meteors in segment A, which escaped both these fates and continued to be ortho-Leonids, that Adams's investigation is concerned. He ascertained their orbit; and starting from the elements of the orbit as determined by him, the actual perturbations which it has since undergone have been computed, and the main results thus arrived at are embodied in the following table.

As already stated, the calculation has been extended over an entire revolution of that portion of the stream which we have called segment A; and in computing the perturbations, account has been taken of the attraction exercised upon these meteors by Mars, Jupiter, Saturn, and Uranus. At first Venus and the Earth were included, but as the influence of these planets was found to be insensible, they were omitted from the latter part of the calculation.

The expense of carrying on the work has been met partly out of the Government Grant administered by the Royal Society, and partly out of the Royal Society's Donation Fund. The computations have been made by Messrs. F. B. Cooper, J. H. Bell, and W. H. Walmsley, members of the staff of the Nautical Almanac office. We are also indebted to Mr. E. Roberts, the chief assistant, for his aid in various parts of the work. The method adopted was that by mechanical quadratures, the determinations of the variations of the elements being made at intervals of thirty-six days, except for the period from May, 1871, to December, 1894, during which time the perturbations were small and progressed so regularly that it was found sufficient to make the computations at intervals of 216 days.

The most noteworthy features are a near approach to Saturn in April, 1870, and a near approach to Jupiter in August, 1898, at which latter time the meteors in segment A of the stream were at a distance from the planet of only 0·9 of the mean radius of the earth's orbit. The consequences of these near approaches are brought out in the table. Uranus produced but little effect in this revolution. The planet was at a distance when the swarm crossed his orbit. And the influence of Mars was trifling. So that nearly the whole of the perturbations during this revolution have been caused by Jupiter and Saturn.

Perturbations of the Elements of the Orbit of Segment A of the Ortho-stream in certain Selected Intervals of Time.
The Elements are referred to the mean Equinoxes of their respective epochs.

		Elements of the osculating ellipse on 1866, November, 13 d. 13 h., as found by Adams.	Perturbations of the elements in the selected intervals.					Computed values of the elements on 1900, January, 27 d. 15 h.
			I.	II.	III.	IV.	V.	
Mean longitude in orbit	ϵ ...	58° 10′·2	− 4′·83	− 0′·32	−27′·98	−13′·99	−0′·70	58° 34′·4
Longitude of perihelion	ϖ ...	58° 19′	− 5′·37	+10′·70	− 6′·47	− 4′·75	−0′·60	58° 40′·6
Longitude of node (descending)	ν ...	51° 28′	+29′·33	+ 7′·15	− 1′·69	+70′·83	+0′·09	53° 41′·8
Inclination	i ...	16° 46′	+11′·92	− 1′·01	+ 1′·43	−28′·60	−0′·01	=16° 29′·7
Angle of eccentricity	ϕ ...	64° 46′·8	− 3′·39	− 1′·70	+12′·06	+ 7′·65	+0′·32	=65° 1′·7
Mean distance	a ...	10·3402	+0·015 660	−0·021 271	+0·033 726	+0·038 258	+0·001 747	=10·408 32
Daily motion of ϵ	n ...	−1′·778 57	+0′·004 069	−0′·005 481	+0′·008 678	+0′·009 763	+0′·000 441	= −1′·761 10

I is the interval from 1866, November 13, to 1871, May 3. In this interval segment A of the ortho-stream crossed the orbits of Jupiter and Saturn.
II is the interval from 1871, May 3, to 1894, December 28. In this interval it crossed the orbit of Uranus, both on the outward and homeward journeys.
III is the interval from 1894, December 28, to 1897, December 30. In this interval it recrossed the orbit of Saturn.
IV is the interval from 1897, December 30, to 1899, May 18. In this interval it recrossed the orbit of Jupiter.
V is the interval from 1899, May 18, to 1900, January 27. This interval brings segment A of the stream back to its descending node.

The following were the adopted masses of the disturbing planets:—

Mars	$\frac{1}{3,093,500}$
Jupiter................................	$\frac{1}{1,047 \cdot 879}$
Saturn	$\frac{1}{3,501 \cdot 6}$
Uranus	$\frac{1}{22,756}$

In consulting the table, it has to be borne in mind that ϵ, which is there designated, in compliance with the usual convention amongst computers, the "mean longitude in the orbit," is in reality the sum of two angles lying in different planes, viz., the longitude of the node + the angle between the radii from the sun to the node and to an imaginary body starting from perihelion at the same epoch as segment A of the meteors, and thenceforward moving uniformly in a circular orbit round the sun in the same plane and with the same periodic time as the meteors. So again π, the so-called "longitude of perihelion," is the sum of two angles, viz., the longitude of the node measured along the ecliptic + the angle from the node to the perihelion measured in the plane of the orbit. The second angle in each case, that in the plane of the orbit, is measured in the direction of positive motion.

The perihelion distance in Adams's orbit, of which the elements are in the first column of the table, and which was the osculating ellipse on 1866, November 13, is 0·9855; that of the osculating ellipse on 1900, January 27, of which the elements are in the last column, is 0·97296. There is a corresponding difference in the distances of the node from the sun, a difference which would be enough to carry segment A of the meteoric stream inside the earth's orbit without intersecting it when it passes the earth's orbit on January 27, 1900, unless the depth of the stream towards the sun is greater than its width at right angles to that direction—a width which from observation has been estimated to be about 100,000 miles. We have, however, satisfied ourselves, from the dynamical conditions which must have prevailed when the Leonids joined the solar system, that the depth of the stream is much greater than its width.

The longitude of the node at the epoch 1900, January 27, would be 52° 25′, if computed in the way which has been hitherto usual, by applying to the longitude at the time of the shower of 1866 the average apparent shift of the node as determined from observation by Professor Newton, viz., 102″·6 annually; whereas in the orbit of our table it is 53° 42′. It thus appears that the amount of this perturbation upon segment A of the stream has been more than three and a half times its *average amount*, and, doubtless, the perturbations in this revolution of

the other elements have also been excessive as compared with their average amounts.

Thus, the mean distance of the meteors occupying segment A of the stream has been undergoing so much extension, that the meteors will at the end of the revolution find themselves with a periodic time longer by one-third of a year—an amount of change which must largely affect their future history, unless this great perturbation is compensated by what happens elsewhere or at other times.

At the epoch 1899, November 15, the longitude of the node will be 53° 41′·7, a position which the earth will reach on 1899, November 15d. 18h. It is probable, therefore, that the middle of the shower of the present year (1899) will occur nearly at this time, since segment A in the stream, for which our calculations have been made, is situated in the stream less than three months' journey of the meteors behind the segment which the earth will encounter next November, and which we may call segment B. This conclusion, however, rests on two assumptions: (1) That segments A and B were, in 1866, moving in orbits that did not much differ; (2) That the perturbations which segments A and B have since suffered have not much differed. Both assumptions are probable, but unfortunately neither is certain; so that the prediction can only be offered with reservation. If the shower occurs at the time anticipated, it will be visible from both Europe and America.

"On Hydrogen Peroxide as the active Agent in producing Pictures on a Photographic Plate in the Dark." By W. J. RUSSELL, Ph.D., V.P.R.S. Received February 18,—Read March 2, 1899.

In previous papers it has been shown that certain bodies are able, in the dark, to act on a photographic plate and produce a picture. The purpose of the present communication is to show that in all the cases which have been examined, and probably in all others of a similar kind, the action which occurs is due to the presence of hydrogen peroxide. As a sensitive plate always contains moisture, and probably would be inactive if quite dry, it does not seem possible to test the truth of this statement by the total exclusion of moisture; therefore more indirect means have to be adopted. In the following paper no attempt is made to explain the reactions which occur in the plate itself; that is a distinct question, and at present the object is to consider the means by which these changes, whatever they may be, are brought about. These changes are rendered visible by exactly the same processes as those adopted for the development of an ordinary light picture. Any of the ordinary photographic plates may be used in

these experiments, but as many of the pictures are only formed after a long exposure, it is well to use rapid plates. In the following experiments the plate used has been in almost all cases the "Ilford special rapid," and the process of development has in every case been that recommended for their ordinary use.

The first step towards demonstrating that hydrogen peroxide is the active agent in producing these pictures, is to show that all the results produced both by metals and by organic bodies on a photographic plate, can be produced by hydrogen peroxide. This body is now made in considerable quantities and sold in aqueous solution of a given strength. This commercial article appears to act equally well to a carefully prepared and pure specimen of the same strength.

A convenient way of testing the action of any liquid on a photographic plate is to use a small circular glass dish, such as is made for bacteriological experiments, the photographic plate resting on the top of the dish, and the amount of the liquid used determines the distance the plate is from the active surface, the experiment being carried on in complete darkness. If pure water be tested in this way, it is found that no picture, that is no darkening of the plate, occurs on its being treated with the developing solution. The plate can be left over the water for eighteen to twenty hours, but if left longer than this, the film is destroyed by the aqueous vapour. If to the pure water in the dish a mere trace of hydrogen peroxide be added, a darkening of the plate will quickly occur. For instance, if the liquid contains only one part of the peroxide in a million of water, and the plate be exposed to its action for eighteen hours, a faint picture is produced. Bearing in mind the small amount of evaporation which takes place under these conditions, and consequently the minute amount of the peroxide which comes in contact with the plate, it clearly shows the exceeding delicacy of the reaction.

Again, if a piece of Ford blotting paper, which by itself is inactive, after being wetted with a solution of one part of peroxide in 500,000 of water, and hung up in a warm room for three quarters of an hour to dry, is placed in contact with a photographic plate for two hours at a temperature of 55° C., on subjecting the plate to development a distinct picture is produced. In fact, moistening good blotting-paper with a solution which may be strong or weak, and allowing it to dry for a long or short time, is a very good way of applying the peroxide. In place of blotting paper any inactive porous substance may be used.

Plaster of Paris wetted with a peroxide solution and allowed to set, continues for a long time to be an active body. If by any of these means a large, in place of a small, amount of the peroxide be allowed to act on a plate, then in place of a dark, a light picture is obtained, a phenomenon similar to what is known to photographers as reversal.

The conditions under which certain metals and certain organic bodies

act on photographic plates, and how pictures of the structure of paper, skeleton leaves, lace, and other bodies can be obtained, has already been described, so that now it is only necessary to say that substitute for these active bodies peroxide of hydrogen, and exactly corresponding results are produced. Writing with ordinary ink, or with a solution of ferrous sulphate, or potassium ferrocyanide, has been shown to be opaque to the action of zinc and of turpentine, so is it to the action of the peroxide of hydrogen. Further, the action exerted by the metals and the terpenes, is unable to pass through glass, mica, selenite, &c., but is able to pass through thin sheets of gelatin, celluloid, gutta-percha, india-rubber, tracing paper, gold beaters' skin, parchment, &c. Peroxide of hydrogen acts exactly in the same way; every body which is known to be either opaque or transparent to the action of the metals or terpenes, is opaque or transparent to the action of the peroxide; so that as far as the production of similar phenomena goes, the agreement is complete. Of the acknowledged tests for the presence of hydrogen peroxide, the one with the titanic acid dissolved in sulphuric acid is exceedingly delicate; so also appears to be the tetramethylparaphenylenediamine paper of Dr. Wurster, and both of them have been made use of.

The next point which naturally suggests itself is, whether peroxide of hydrogen is, or is likely to be, present in all the different cases, when action on the sensitive plate occurs. First, with regard to the metals. The list of the active metals, which has already been given, is as follows, arranged approximately in order of their activity: Magnesium, cadmium, zinc, nickel, aluminium, lead, cobalt, bismuth, tin. Now these are certainly the metals which might be expected to decompose water, and in the presence of oxygen cause the formation of hydrogen peroxide; and still more the order in which they stand in the above list, judging from their general properties, is that in which they would probably induce the formation of the peroxide. It is also satisfactory to note that this list of metals and their order of activity was drawn up simply from experiment, when there was no idea that hydrogen peroxide had anything to do with the reaction. Again, as a confirmation that hydrogen peroxide is formed when these metals oxidise in moist air, pieces of Dr. Wurster's tetra-paper were moistened and laid on bright surfaces of the metal. With the metals that head the foregoing list a considerable amount of blue colour was rapidly developed, with the metals at the end of the list the amount of colour was less, and the reaction slower; and with other metals, such as silver and platinum, there was no action. With copper and with iron a very slight amount of action did occur, but these metals do not appear able to produce definite pictures. Iodide of potassium and starch paper, when used in the same way, gave a blue colour with all the active metals, but none with copper nor with iron.

On the supposition that hydrogen peroxide is the active agent in the action exerted by the metals, it seemed probable that on supplying to the metal more moisture than it obtained from the air and photographic plate, more action would take place, and this was found to be the case. Two pieces of polished zinc were placed in contact with photographic plates in small iron boxes; one box was quite dry, and put in a bell-jar over calcium chloride, and into the other box some damp paper was introduced, and the box was placed with a little water under a bell-jar. On examining the plates after three days it was found that the damp plate had much the darker picture on it.

With the object of obtaining an increased amount of action, experiments were made by passing a current of warm moist air over zinc turnings. A glass tube, 6 feet long and 1 inch in diameter, was packed with zinc turnings, and placed within a large brass tube to which steam could be admitted. The amount of action, if any, was indicated on a plate, placed in a dark box at the end of the tube. Even under the most favourable conditions no very large amount of action took place. When a current of moist warm air was passed through the tube for an hour a fairly dark picture was obtained. If the air was dry no picture appeared. If amalgamated zinc was used in place of pure zinc a darker picture was formed, and, as a check on these results, dry and moist air, both warm and at ordinary temperatures, was passed through the tube, no zinc being present, and then no action took place. Also when ozonised air was passed through the tube there was no action. Passing now from the metals to the organic bodies capable of acting on a photographic plate, it has been found that they belong essentially, if not solely, to that class of bodies known as terpenes, and it is a general property of all this class of bodies when oxidising to give rise to hydrogen peroxide. Thanks to Dr. Tilden, experiments have been made with most of the terpenes, and all were found to be very active bodies; both pinene and limonene were tried, in their dextro- and lævo-rotatory state, but their activity appeared to be the same. Oxidised and other compounds connected with the terpenes, such as terpineol camphor, thymol cymene, have no power of acting on a photographic plate, but ordinary turpentine and terebene are very active bodies.

Most of the ordinary essential oils, such as bergamot, peppermint, pine, lemons, cajuput, &c., have been experimented with, and, without exception, have been found to be active bodies. It is well known that they all contain terpenes. They are also characterised by a strong odour, and as ordinary scents contain some of these bodies, it follows that almost all vegetable bodies having a strong smell are capable of acting on a photographic plate. Eau de Cologne gives a good picture, so do many wines and brandy, and coffee, guaiacum, cinnamon are also *active substances*; thus the photographic plate becomes a very delicate

test for the presence of all these bodies, and as the action is cumulative it may even compete with the sense of smell.

In addition to the essential oils, the ordinary vegetable oils, such as linseed oil, which is the most active, and colza and olive, which are much less active and have much less power of absorbing oxygen from the air, can act on a photographic plate. The tetra-paper readily goes blue if suspended in a bell-jar which has a few drops of linseed oil in a dish within it.

The mineral oils are, on the contrary, devoid of this power of acting on the sensitive plate, and the same applies to bodies such as benzene, phenol, naphthalene, aldehyde, methyl alcohol, coal naphtha, &c.

It would seem, then, that all the organic bodies capable of acting on the photographic plate are capable of giving rise to the formation of hydrogen peroxide when they oxidise in moist air.

In former papers it has been shown that the active bodies, both metallic and organic, are able to act on a photographic plate even when thin layers of many different substances are interposed; for instance, if a thin sheet of gelatin be laid on a polished zinc plate it only very slightly modifies either the sharpness of the picture or the time required for its production. If the gelatin plate be thicker the action will still pass through, but the picture will be more indistinct, and the time necessary for its production longer. If a 2 per cent. solution of hydrogen peroxide be poured into one of the small glass dishes, and a sheet of gelatin 0·0013 inch thick be placed over it $\frac{1}{8}$ inch above the liquid, a picture will be obtained in fifteen minutes. If the sheet of gelatin be 0·008 inch thick, then the exposure must be for one hour; and if the gelatin be 0·01 inch thick, an exposure of three hours is necessary. If a sheet of celluloid be substituted for the gelatin, and it be 0·005 inch thick, the action still passes through, but more slowly than through the gelatin, and the plate now requires one hour exposure to give a good picture. With a plate of celluloid of double the above thickness, the exposure must be four times as long; and if the thickness be 0·033 inch, the time of exposure has to be thirty hours. These determinations show well what happens in these cases, but are only good approximations, not standard results. In addition to gelatin and celluloid, guttapercha tissue, india-rubber, tracing paper, collodion, albumin, gold beater's skin, parchment, &c. also allow the action to take place through them, and the obvious question which presents itself is, If hydrogen peroxide be the body which gives rise to the action, how does it pass through these different bodies? Take the definite case of zinc; if a plate of this metal be rubbed with coarse sand paper and placed in contact with a photographic plate, a clear and sharp picture of the scratches is obtained, and it might have been expected that when the action took place through even a very thin sheet of gelatin the picture of the scratches

would have no longer been visible, or at least only indistinctly so, but experiment shows this is not the case. How then does the peroxide permeate the gelatin? Not by the ordinary process of diffusion, for hydrogen cannot diffuse through it, so that it must be by a process of dissolving, or very feebly combining with the medium, or with a constituent of it, and thus travelling through escape on the other side. That the action is of this nature seems rendered probable by the following experiments, which show, at least to some extent, what takes place.

A 2 per cent. solution of hydrogen peroxide was placed in a dish with a sheet of the thinnest gelatin, about one hundredth of an inch thick; above it and on the gelatin a photographic plate was placed, and allowed to remain there for twenty minutes. No picture was formed. Immediately on removing this first plate from the gelatin, a second one was put in its place, and allowed to remain there also for twenty minutes. This plate gave a faint picture, the third one gave a darker picture, and the fourth one was still darker; but the fifth, sixth, and seventh plates were, as far as could be judged by the eye, of the same degree of darkness. Thus the amount of peroxide given off on the upper surface of the gelatin went on increasing for one hour and twenty minutes, and then the action became uniform. The same kind of action occurs if zinc be used in place of peroxide solution. If a thin sheet of gelatin be laid on a piece of zinc and allowed to remain there for a week, then, on placing above it a sensitive plate, a picture will be produced in about one-third to one-fourth the time which would have been necessary if the previous exposure to the zinc had not taken place. Celluloid was found to act exactly in the same way as the gelatin. The plate, after the first half-hour's exposure, gave no pictures, but a faint one after the second half-hour; and it was not till after the fourth half-hour that the action became constant. A thicker specimen, 0·011 inch thick, was also examined after intervals of two hours, it acted in the same way as the other specimens, but required ten and a half hours before the action became uniform. If drying oil or copal varnish be used in place of the peroxide of hydrogen solution, analogous results are obtained. This action explains how pictures can be obtained from invisible originals. If, for instance, a piece of white cardboard or paper is placed behind a copper stencil and is exposed to the vapour from peroxide of hydrogen solution, drying oil or copal varnish, &c., the exposed part of the paper becomes active, although not visibly affected, and on placing it on a sensitive plate, a picture of these parts is produced. Zinc acts in the same way, but only slowly. A zinc ornament, laid on a piece of Bristol board for eight months, charged the board only so far as to enable it to give a faint picture.

Gelatin can be substituted for the paper in these experiments, and *can be charged* and made to convey a clear picture to a sensitive plate.

It is then evident that the action arising from zinc and other active bodies can, by an intermediate and inactive substance, be carried away and allowed to expend itself at another time and at another place.

With regard to the transmission of the action through gelatin, the water which it contains is probably the body which enables the peroxide to pass through. It can also be shown that it aids the transmission of the action through other inactive bodies, for instance if Bristol board in its ordinary condition be placed on a polished piece of zinc, the action of the zinc only slowly passes through it, but if the board be damp the transmission takes place much more rapidly. The following comparative experiments illustrate this. Two similar pieces of Bristol board were taken, one was dried and then placed between a piece of perforated zinc and a sensitive plate and put under a bell jar with calcium chloride; the other piece of Bristol board was suspended over water until it was thoroughly damp, and then placed between perforated zinc and a sensitive plate under a bell jar with a little water present. Both experiments were continued for twelve days, when it was found that with the dry board there was no picture produced, but with the damp one there was a good and dark one. If copal varnish be used in place of zinc, similar results are obtained, and if parchment be substituted for Bristol board the results are the same.

These experiments are however not conclusive, for it has been shown that with additional amount of water some of it finds its way to the zinc, and there induces the formation of more peroxide which may account for the darker pictures. Even with the terpenes the additional amount of water may induce the additional formation of peroxide. This objection can however be obviated by cutting off the moisture in the damp medium from the active substance, or by using the aqueous solution of the peroxide as the origin of the action. In order to stop the aqueous vapour from either passing from the damp Bristol board or to it from the peroxide solution, a piece of tracing paper is interposed which allows the action to pass through it, but not any appreciable amount of aqueous vapour. On placing a sheet of tracing paper over a glass dish containing the peroxide solution and above it dry Bristol board with a photographic plate, in one and a half hours just an indication of a picture was produced, but when under the same conditions Bristol board which had been over water for nineteen hours was used, then a dark picture was formed. Again similar experiments were made using a not highly glazed paper in place of the Bristol board, and the results were the same.

In place of tracing paper, celluloid was used and the dry Bristol board gave, under similar conditions, no picture, but the damp one gave a very distinct picture. In order to avoid having so much water present, plaster of Paris set by a little of the peroxide solution was used in place of the aqueous solution, and exactly similar results were obtained.

so there is no doubt that hydrogen peroxide can readily pass through a porous body by the aid of water.

Alcohol acts in the same way as water, for when plaster of Paris wetted with peroxide solution was poured into a couple of similar dishes and allowed to set, and over one a piece of dry and over the other a piece of Bristol board moistened with alcohol were placed, and sensitive plates above them, after fifty minutes only a very faint picture was formed above the dry board, but a dark one over the wetted board.

Celluloid is however nearly as transparent to these actions as gelatin, and water in this case cannot be the transmitting medium, so that the question is whether there be any constituent of the celluloid which may act in a similar way to that of water in the gelatin. From the following experiments it seems that camphor can do so:—

Camphor itself like water is a perfectly non-active body. To obtain a thin non-porous layer of this body is difficult, but it is easy to prove that the emanations from hydrogen peroxide solutions, from zinc, copal, or other active bodies, are readily absorbed by it, and readily pass through it. For instance, if a piece of camphor be placed about quarter of an inch above a 2 per cent. solution of hydrogen peroxide for seventeen hours and be then removed and placed on a sensitive plate for fifteen minutes, it gives a dark picture, and when a similar experiment is made using drying oil in place of the peroxide solution, and the camphor be exposed to its action for three days and then brought in contact with the sensitive plate for one day, a dark picture is produced. This action can however be easily carried still further and proved to pass through even a thick layer of camphor. A piece 0·137 inch thick was placed about $\frac{1}{8}$ inch above a 2 per cent. solution of peroxide in a dish, for sixty-six hours, and a sensitive plate placed on the top of it; on treating this photographic plate with the developing solution it was found that a considerable amount of action had occurred. Thus the camphor which is a principal constituent of celluloid may enable hydrogen peroxide to pass through it.

That guttapercha and pure india-rubber should allow the action to pass through them is remarkable. The substance known as guttapercha tissue has a thickness of about 0·003 inch, and allows the action to pass readily through it; in fact, if even two thicknesses of this tissue be placed over the 2 per cent. solution of the peroxide for seventeen hours, a dark picture is obtained. If the tissue be laid on a polished piece of perforated zinc and a sensitive plate above it, after remaining there for a fortnight a fairly good picture is obtained. If drying oil be used, the action will pass through the guttapercha in three days. With regard to this transmission of the action, although the chemical constitution of guttapercha is not well established, it is said to be a

body related to camphor,* and hence the action passes through it as it does through celluloid, and this is borne out by the fact that if a piece of guttapercha be placed for eighteen hours over the 2 per cent. peroxide solution, and then placed for twenty minutes on a sensitive plate, it evidently has become active, for it then gives a good picture.

The above remarks apply also to india-rubber. The thinnest sheet that has been experimented with is 0·017 inch thick; this allowed the action to pass through it, but was too thick to give a picture, but, like the guttapercha, if placed over the peroxide solution it became active, and produced considerable action on a photographic plate.

With regard to other substances which allow the action to take place through them, the most interesting are true gold beater's skin, and albumen. If Bristol board or paper be carefully painted on one side with white of egg and allowed to dry in the air, it forms a medium through which the peroxide can pass. Collodion also allows the action readily to pass through it. In all these cases the tetra-paper may be used to confirm the results obtained.

Then with regard to bodies which do not allow the action to pass through them. Paraffin is one of them. If paper be painted with melted paraffin and it be placed over a solution of the peroxide, no action passes through, neither is it able to absorb the peroxide like camphor and india-rubber and guttapercha. A piece of paraffin placed over the peroxide solution for twenty hours and then tested by placing it on a sensitive plate produced no action.

Gum arabic is a body which sometimes is very opaque, but this is simply a question of hydration, and is confirmatory of what has been said before with regard to the action of water. Some unglazed paper was painted on one side with two coats of good gum arabic, and some of it was dried at 55° for some days, and another portion of it was *air*-dried only for some hours, and both were put over drying oil for three days. The dried paper gave only a very faint picture, but the more moist one a very dark picture.

When experimenting some time ago on the general nature of these reactions, polished zinc was placed below some inactive liquids to test whether any action took place through them. The small glass dishes were used, and a disc of bright zinc laid inside, and the liquid to be tested poured upon it; then the photographic plate was placed on the top of the dish. After remaining there for three or four days the plate was generally found acted on as if the zinc had been able to exert its influence upon it. Lately these experiments have been repeated and extended, and, as indicating the extreme delicacy of the reaction with the photographic plates, are of interest. The form of experiment was the same as described above, and the liquids used were alcohol, ether, ethyl acetate, chloroform, benzene, petroleum spirit. All these liquids

* Bernthsen, 'Organic Chemistry,' p. 509.

were purified, so that when placed in the dish with the sensitive plate above them no action after a week's exposure took place. However, when a zinc disc was introduced below the inactive liquids the photographic plate was generally acted on, but with the benzene and petroleum spirit sometimes no action occurred. These rather singular results were next tested in another way. Portions of these inactive liquids were put into stoppered bottles with polished strips of zinc foil, and allowed to remain there for a week, and it was then found that the liquid had become active, for on testing it by putting it into a dish with a photographic plate above it, a dark picture was formed, so that the action of the zinc was to make the whole of the liquid active. Magnesium, cadmium, aluminium, fusible metal, and bismuth, all produced effects similar to those obtained with the zinc, but nickel, lead, tin, &c., produced no such effects. Further it was proved that a very small amount of peroxide rendered alcohol, for instance, very active; 0·1 c.c. of a 2 per cent. solution of the peroxide added to 10 c.c. of alcohol gave it the power of acting on a sensitive plate $\frac{1}{8}$ inch above its surface, so as to produce in a few hours a dark picture. The still more careful purification of these liquids, and especially the exclusion of moisture, was undertaken, and in every case it was found when all moisture was excluded that the zinc had no longer the power, when below a liquid, of acting on a photographic plate. Specimens of alcohol, ether, and chloroform were prepared, and these when placed in a dish with zinc at the bottom of it (standing over sulphuric acid) allowed no action to pass through them, and when treated for a week or more with bright zinc in a bottle still retained their perfect inactivity. To a sample of the alcohol which in a dish with zinc allowed no action on the sensitive plate above to occur, a trace of water was added, as much as adhered to the end of a thin glass rod, and now with the same length of exposure a dark picture was formed. From these experiments, as well as those previously mentioned, it appears that this action on the photographic plate is one of extreme delicacy.

The action of water alone on zinc is interesting, and appears to confirm the view that hydrogen peroxide is the active agent in all these reactions. It has already been shown that although bright zinc is active, dull zinc is inactive. However, if a piece of bright zinc be placed in water and remains there for twenty-four hours or so, it, of course, oxidises, white spots or lines appear, and, in fact, in time the whole surface would become covered with oxide. Now the oxide thus formed is strongly active. Take the plate out of the water, let it dry, place it in contact with a photographic plate, and a strong picture of the spots of oxide is obtained. No doubt peroxide of hydrogen is formed, and remains entangled in this porous oxide; in fact it is difficult entirely to remove it. The plate, with this oxide on it, may be dried at ordinary temperatures and exposed to the air for a day or two, and

the oxide is still active, or it may be dried over calcium chloride or even exposed to a vacuum for some time, and it is still active, but if heated to 55° for seventeen hours then its activity is gone and a picture the reverse of the former one is obtained, that is, the oxide is now quite inactive, but the metal itself is very slightly active. Oxides of zinc, cadmium, and magnesium, if wetted with peroxide of hydrogen solution, act in the same way and retain their activity with great pertinacity.

From the foregoing experiments it is then concluded that hydrogen peroxide is the agent which directly or indirectly causes the changes in the photographic plate.

This investigation has been carried on in the Davy-Faraday laboratory, and I would again tender my best thanks to the managers of the Royal Institution for allowing me to work there. My thanks are also due to Mr. O. F. Block who has most efficiently helped in carrying on the above experiments.

March 9, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A Preliminary Note upon certain Organisms isolated from Cancer, and their Pathogenic Effects upon Animals." By H. G. PLIMMER. Communicated by Dr. ROSE BRADFORD, F.R.S.

II. "On the Gastric Gland of Mollusca and Decapod Crustacea; its Structure and Functions." By Dr. C. A. MACMUNN. Communicated by Professor M. FOSTER, Sec. R.S.

III. "On the Structure and Affinities of *Matonia pectinata*, R. Br., with Notes on the Geological History of the Matonineæ." By A. C. SEWARD, F.R.S.

IV. "A Sugar Bacterium." By Professor MARSHALL WARD, F.R.S., and Professor REYNOLDS GREEN, F.R.S.

V. "Note on a new Form of Light Plane Mirrors." By A. MALLOCK. Communicated by LORD RAYLEIGH, F.R.S.

"On Flapping Flight of Aëroplanes." By MAURICE F. FITZ-GERALD, Professor of Engineering, Queen's College, Belfast. Communicated by Professor G. F. FITZGERALD, F.T.C.D., F.R.S. Received February 15,—Read March 2, 1899.

It has been long known, principally through experiments on soaring, that a large, if not by far the largest, part of the supporting force obtained by birds in regular flight, is probably furnished by upward air pressure on their wings, regarded as planes moving horizontally, with their surfaces slightly inclined to the direction of motion.

Langley's "Experiments on Aëro-dynamics" furnish some numerical data for estimating the power required to sustain an aëroplane of given weight, propelled horizontally by a known force, and he applies these to the determination of the problem whether this could be effected by screw propellers, analogous to those of a ship, actuated by machinery of existing type.

The older mathematical investigation by Navier of the problem of flapping flight, seems to be quite discredited, and, indeed, the results brought out always laboured under the physiological difficulty of demanding an amount of horse power per pound of muscle in birds which surpassed, to an almost incredible extent, that available from the better known muscular tissues of other animals. One horse power is 550 ft.-lbs. per second; a horse can ordinarily exert about two-thirds of this, and if we take him to weigh about 10 cwt., we get about a third of a ft.-lb. per second per pound of horse. A man can work at the rate of about 50 ft.-lbs. per second; this, again, is about a third of a ft.-lb. per second per pound of man; and so on with other animals. But, for the flight of birds, it was made out that from about 30 to 1300 ft.-lbs. per second per pound of bird were necessary, which, even after all allowance for higher temperature of their bodies, and large relative mass of wing muscles, compared with those of the limbs of horses, men, &c., seemed highly improbable.

In the following paper an attempt is made to indicate how both progressive and hovering flight may be effected by aëroplanes, attached to a heavy mass, and flapped after the manner of wings, under conditions sufficiently nearly approaching those of Langley's experiments to justify the inference, from his figures, of numerical results, not indeed presumably exact, but sufficiently indicating the order of magnitude of the quantities involved. The figures given may be taken, commercially speaking, as near enough to the truth to enable us to judge whether we are dealing with pounds or with pence, though not exact enough to adjust the change out of half-a-crown in paying for our power.

Let, then, a heavy mass be supposed to be flying through the air

horizontally, and be supported by wings, consisting of planes of negligible mass, held nearly edgewise to the direction of motion, and moved vertically up and down by some machinery carried by the heavy mass. Let there be in addition some arrangement of the machinery by which the angle of inclination of the planes or wings to their direction of motion is variable. We shall suppose the velocity of progression high, and variations of the propelling force small enough for changes of forward velocity to be neglected, compared to the average forward velocity. This is justified by Langley's experiments, which show that, for small inclinations of an aëroplane, the direct resistance to forward motion is small compared to the supporting force. We shall also, in the first instance, neglect direct air resistance on the heavy mass as small compared with that on the aëroplanes; and, for convenience, we shall call the heavy mass the bird's body and the attached aëroplane the wings.

The case differs from that of a real bird most notably by the circumstances that the aëroplane in our theoretical case has no mass, whereas, a bird's wings have one of very sensible magnitude compared with that of its body, and the aëroplane is supposed to be moved up and down, relatively to the mass, as a whole, instead of being pivoted to it, as a bird's wings are to its body, besides being of constant area, which wings are not. Consequently, as before remarked, numerical results can only be regarded as indicating the order of magnitude of the quantities calculated, not their exact values.

The inclination of the wings being variable, and their motion being compounded of an up-and-down one with a forward one, it is evident that the supporting force may be periodically variable also, and consequently the bird's body will move in a sinuous or wavy path, on the whole horizontal, and the stroke of the wings will be the relative motion of wings and body. If the horizontal velocity be high, and the amplitude of the wing stroke relatively to the air moderate, and the variations in the supporting force not too great, the path of the bird's body will be only slightly waved, so that fig. 1 may be taken as, in a general sort of way, representing what takes place.

In the same figure A_1B_1, A_2B_2, &c., represent the plane of the wing at different successive positions, seen edgewise, and P_1, P_2, &c., the corresponding resultant air pressures on it, in direction and magnitude. Now it is plainly a matter of arrangement of mechanism what these shall be, as they depend on the angle between the lines A_1B_1, &c., and the tangent to the heavy line, marked "Path of wing" at each instant. Consequently it is worth while inquiring what are the conditions of adjustment to cause a forward force on an average to be applied to the bird and wings, when the wings are moved up and down by an engine forming part of the bird's body, when the supporting force is, on an average, equal to the bird's weight, and the forward

force is, on an average, equal to that required, on an average, to propel the aëroplane at its average inclination. Observe that the force P_n depends for its magnitude, not on the actual inclination of the wing path at all, but on the angle between that path and the plane of the wing, while its horizontal component depends on both angles, so that although, as long as there is any supporting force, there is a resistance to forward motion along the wing path, there may nevertheless be a forward force acting horizontally on the whole machine, wings and body taken together, as in the first position shown, for instance.

The thing to do, then, is to formulate this in symbols, and for the present purpose it will suffice to make some simplifying approximations to facilitate the work. We shall then take it that the inclination of the wing path to the horizontal, and the inclination of the plane of the wing to the wing path, are both small enough to assume that for either of them, as well as their sum, the circular measure, the sine, and the tangent do not sensibly differ, and the cosine is unity, for example, for 20° circular measure = 0·349, sine = 0·342, tangent = 0·363, and cosine = 0·94; so that, up to this at any rate, we shall not be incurring errors exceeding 5 or 6 per cent. by this assumption.

We shall also assume that for a small angle (α) of inclination of an aëroplane to its path, the resultant air pressure is given by the formula $P_\alpha = 2kV^2 \sin \alpha$ in pounds per square foot, where V is velocity in feet per second, and k a coefficient, which, according to Langley, is about 0·0017, and P_α is directed normally to the plane. This agrees nearly with Du Chemins's formula at small angles, as pointed out by Langley.

Now let the angle α be varied according to the law

$$\alpha = m(1 - \mu \cos pt),$$

m and μ being arbitrary constants, p being $2\pi f$ where f is the frequency or number of flaps per second, t being time in seconds.

Then the normal pressure, P_α, on a plane of area A, at forward velocity V, inclined at α to its direction of motion is

$$P_\alpha = 2kV^2 A m(1 - \mu \cos pt).$$

For convenience we shall take the bird's weight as 1 lb., so that, in general, P_α will be, for any other weight, normal pressure in lbs. per lb. of bird, and A is wing area in square feet per lb. of bird.

Let, at any moment, Z be the height above some datum level of the bird's centre of gravity, and z that of centre of pressure of wings, and suppose that

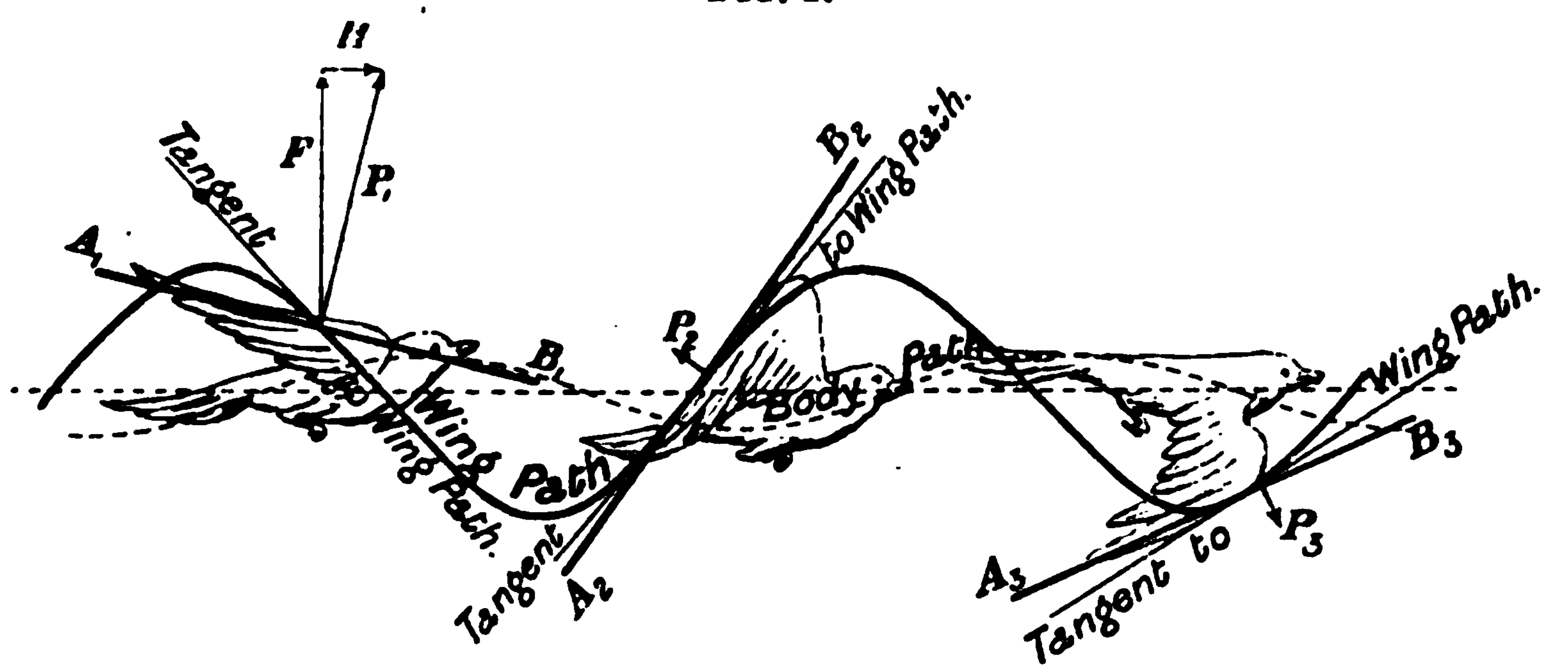
$$z = S \sin (pt + \theta).$$

Then the slope, s, of the wing path is at any moment sensibly

$$s = \frac{pS}{V} \cos (pt + \theta)$$

supposed to be always a small angle.

Fig. 1.



By fig. 1 we see that if F be the vertical supporting force,

$$F = P_a \cos(s+\alpha) = P_a \text{ sensibly},$$

since $(s+\alpha)$ is supposed a small angle; and if H be the horizontal component of P_a,

$$\begin{aligned} H &= -P_a \sin(s+\alpha) \\ &= -P_a(s+\alpha) \text{ nearly.} \end{aligned}$$

This is the resistance to forward motion horizontally, and the work done against it, taking the average rate of working, is

$$U = \frac{p}{2\pi}\int_0^{2\pi/p} VP_a(s+\alpha)dt \text{ ft.-lbs. per second.}$$

Again, if the bird's body be rising at the rate dZ/dt, and the wings at a lesser rate, dz/dt, the engine attached to the body, which exerts a downward force, F, on the wings, is doing work, so that, when F is positive, and $\frac{d}{dt}(Z-z)$ positive, the engine does work, and this is to be the same work, on the average, as that done to overcome the resistance of the air to the plane taken along its actual path, and is

$$W = \frac{p}{2\pi}\int_0^{2\pi/p} P_a \frac{d}{dt}(Z-z)dt.$$

Consequently we get, as a condition for determining some relations among the constants,

$$\int_0^{2\pi/p} VP_a\alpha dt = \int_0^{2\pi/p} P_a\frac{d}{dt}(Z-z)dt \text{ ....(A)}$$

Another condition arises as follows. As F, the vertical force, is sensibly equal to P_a, we have, for the motion of the bird's body,

$$\frac{d^2Z}{dt^2} = g(P_\mu - 1), \text{ the bird being 1 lb. weight,}$$

$$= g\{2kAV^2m(1-\mu\cos pt)-1\}$$

and, on integration, all terms containing t^2 or t as factors must vanish, in order for the motion to be, on an average, horizontal. That containing t^2 as a factor will not vanish unless

$$2kAV^2m = 1 \text{ which fixes } m, \quad \text{(B)}$$

and gives $\frac{dZ}{dt} = -\frac{\mu g}{p}\sin pt$ as there is no term with t as factor in Z.

By differentiation $\frac{dz}{dt} = pS\cos(pt+\theta)$,

whence $$\frac{d}{dt}(Z-z) = -\left\{\frac{\mu g}{p}\sin pt + pS\cos(pt+\theta)\right\}.$$

Inserting their values in (A) for the quantities involved, we get

$$\int_0^{2\pi/p}\frac{V}{2kAV^2}(1-\mu\cos pt)^2 dt$$

$$= -\int_0^{2\pi/p}(1-\mu\cos pt)\left[\frac{\mu g}{p}\sin pt + pS\cos(pt+\theta)\right]dt.$$

On integration, the only terms that do not vanish give the equation

$$\frac{1}{2kAV} + \tfrac{1}{2}\cdot\frac{\mu^2}{2kAV} = \tfrac{1}{2}\mu pS\cos\theta,$$

which is a quadratic for μ, namely,

$$\mu^2 - 2kAVpS\cos\theta\mu + 2 = 0 \quad \text{(C)}$$

Taking the integral of one side alone we find

$$W = \frac{\mu pS\cos\theta}{2} \quad \text{(D)}$$

Eliminating μ from these we get

$$pS\cos\theta = \pm\frac{W}{\sqrt{(kAVW-\frac{1}{2})}} \quad \text{(E)}$$

If now we draw a curve whose ordinate, y, is $pS\cos\theta$, and abscissa, x, is W, we find it to be of the form shown as AB or CD in fig. 1, with a symmetrical branch below the axis of x, not shown in the figure. *On account* of the steepness of the first part of the curve CD, that

part of it which extends from $W = 2{\cdot}5$ to $W = 3$ is shown with the scale of abscissæ magnified fifty times, as AB, and corresponding curves for μ are given. The case taken is when $AV = 60$, so that it applies to flight at 60 feet per second, if the wing area is 1 square foot per pound carried, 30 feet per second with an allowance of 2 square feet per pound, and so on.

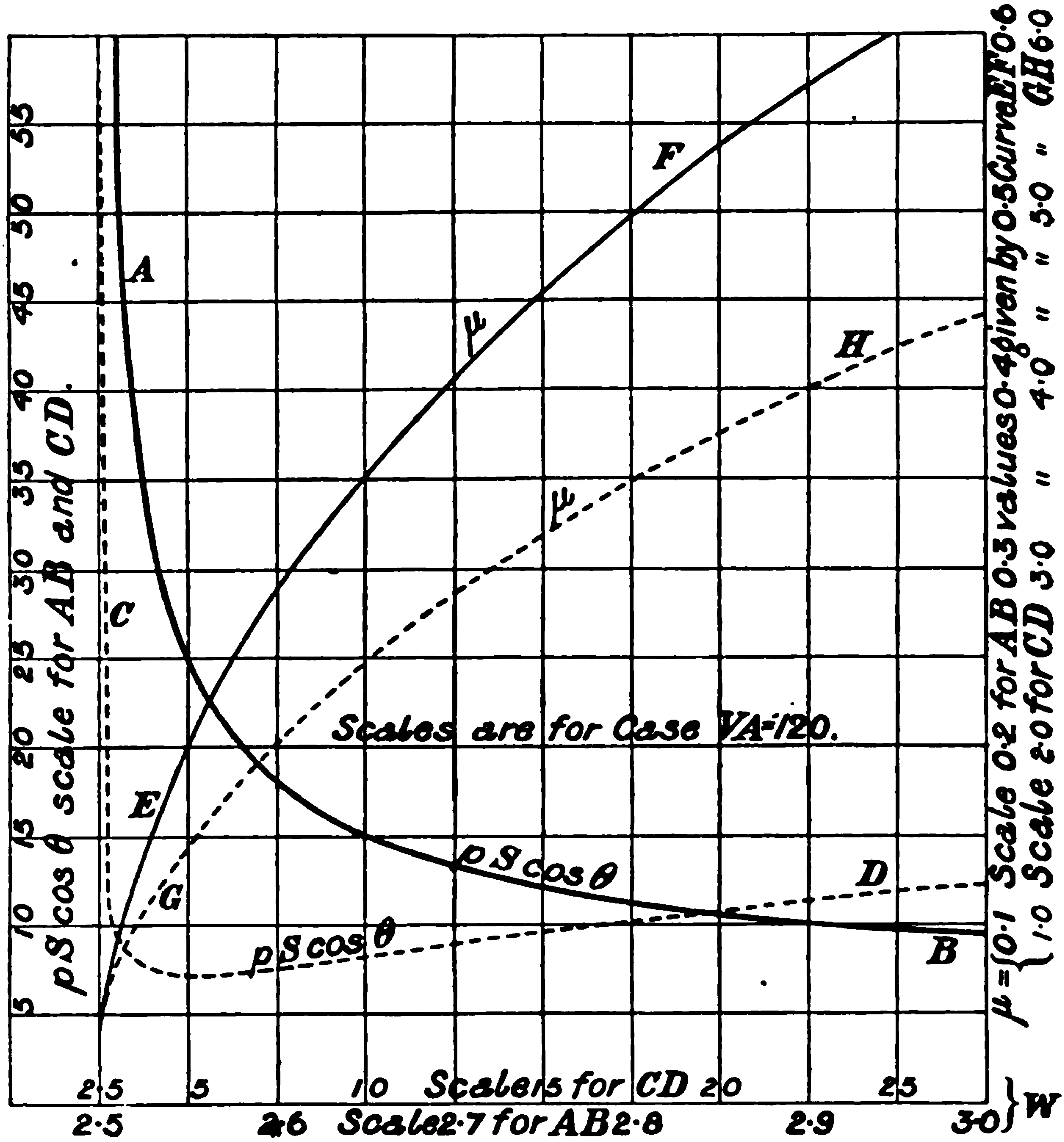
The principal things to note about the curves are that, in the first place, W has a minimum value, in this case 2·5 ft.-lbs. per second per pound carried. This occurs when $pS\cos\theta$ is infinite, and practically implies that when the rate of flapping is very high, keeping the same stroke, the horse-power required comes very near the minimum, and, remarkably enough, the wing becomes, virtually, exactly equivalent to a plane of area A, inclined at a constant angle $a = m$, moving horizontally, as in Langley's experiments. This is not accidental, but seems to follow from the circumstance that $2W = \mu pS\cos\theta$, and if $pS\cos\theta$ be infinite and W finite, μ must vanish, and, consequently, $\alpha = m(1 - \mu\cos\theta)$ becomes $\alpha = m$ simply.

[*Note added March* 21, 1899.—In the paper as read an erroneous statement here followed, to the effect that the minimum value of W was half that got in Langley's experiments with a plane at angle $\alpha = m$. It arose from the left-hand side in equation (A) being, by mistake, written $\int_0^{2\pi/p} VP_a(s+\alpha)dt$, instead of as given above. The author is indebted to Prof. J. Purser for pointing out the mistake. The least value of W is equal to that of Langley's experiments.]

Of course, as before mentioned, it is not to be taken that the figure 2·65 ft.-lbs. per second per pound of bird, is here demonstrated to be that which a real bird, with 2 square feet of wing per lb. of bird, flying at 60 feet per second, would or must exert. All that is professed to be shown is, that the bird may be able to fly with the exertion of two or three ft.-lbs. per second per pound, and that 30 to 1300 are quite unnecessary. Further, it appears that, within a very large range of the values of $pS\cos\theta$, that is frequency of flapping, stroke, and lag of phase of stroke relatively to phase of bird's body motion and twisting of wing, there may be little comparative variation in the horse-power —*e.g.*, between frequency per minute 500 and frequency 250 (supposing $S\cos\theta = 1$), there would only be a change of work per second from 2·51 to 2·55—scarcely 2 per cent.

This seems to bear on the explanation of the great range of variation in the manner of birds' use of their wings; some (as pigeons and ducks) flapping very fast, others (as seagulls and herons) flapping but slowly, without any physiological reason for expecting material differences in the rate at which their muscles can give out energy.

FIG. 2.



Abscissæ W Foot pounds per pound of Load carried.

Ordinates {AB and CD are $pS\cos\theta$. EF and GH " μ

There are, no doubt, plenty of other reasons, but until experiments can be made on planes or other surfaces flapped by pivoting on an axis, numerical data for conditions of calculation agreeing more nearly than those here used with conditions of fact, seem to be wanting.

Another point brought out is that $pS\cos\theta$ has a minimum value as well as W, but μ has not. This means that the phase of the wing motion, relatively to the twisting of the wing (on which the angle α depends) is important, and if these are out of beat, so to speak, by more than a certain amount, with a given frequency and stroke, the flight cannot be maintained at all. The values of μ which exceed

unity imply (on reference to the equation $\alpha = m\,(1 - \mu \cos pt)$) that there is a reversed pressure on the wing during part of the stroke, as shown in fig. 1 at the third position, where P_3 is directed partly downwards. This condition is however unfavourable, probably in all cases, to economy of labour, though it may be favourable to forward propulsion. At all economical rates of working μ is quite small, that is, the inclination of wing plane to wing path varies but little.

It may also be observed that, for any value of $pS \cos \theta$, there are two of W, one of which lies close to its least possible value, and the other is very large—*e.g.*, for $pS \cos \theta = 15$, $W = 2{\cdot}65$ or about 40, so that a person who was going on observed values of pS and took $\cos \theta$ to be nearly unity, might easily overlook the small value of W, and base his estimate of work on the large one. This is the more likely as the curve of $pS \cos \theta$ is of the third degree, and the solving of a cubic by a direct process, especially when there are three real roots, is troublesome, and avoided in consequence. Whether anything of this kind occurred in Navier's or other earlier investigations, the present writer is unable to say. Langley says that Navier added the work done, here expressed by the right-hand side of equation (A) to that on the other to find the whole. Why it should have seemed necessary to do this is not clear, for if the wing has no sensible mass, as here assumed, the pressure of the engine downwards cannot exceed F, and no work can be done by a vertically moving engine, except that expressed on the right-hand side of equation (A). The way in which forward motion is maintained bears some analogy to the process of so-called "invisible" skating, and to screw propulsion, wherein it would be manifestly absurd to add the work done against the ship's resistance to that given by the turning moment applied to the screw, to find the horse-power of the engine. In fact, the construction of Langley's whirling table virtually made the aëroplane a portion of a screw propeller with axis vertical, and his coefficient k has, bound up in it, the inefficiency of the propeller in converting horse-power of turning couple into thrust.

With regard to the absolute magnitude of the figure $2{\cdot}5$ as the least value possible for W in the case illustrated by fig. 2, it is of importance to note that this depends on m, and m has been made $= \dfrac{1}{2kAV^2}$ on the strength of the equation

$$\frac{d^2Z}{dt} = g(P_a - 1) = g\{2kAV^2m(1 - \mu \cos pt) - 1\}$$

which again depends on the assumption that the downward acceleration of the body attached to the aeroplane would be g, irrespective of the horizontal velocity of the plane, if the angle of inclination of the plane to its path were zero. Langley's experiments on the "Plane

Dropper" seem to show that this is by no means the case, but, unfortunately, he was unable, through uncontrollable circumstances, to determine the proper value, which is some function of the velocity, and may possibly be so small as $g/3$ at his highest velocities. If so mV which is the minimum value of W requires corresponding reduction, and, in the case taken, the scale of W is about three times too large, reducing the necessary work to about 1 ft.-lb. per pound carried or thereabouts. It is now evident how the energy expenditure of birds may lie within quite reasonable limits, though we cannot, with any approach to accuracy, make the calculations for the design of a flying machine with flapping wings. We can say it will not in a given case cost a pound an hour for power, but we cannot tell whether it will cost only sixpence, or as much as half-a-crown or three shillings.

To allow for head resistance of the bird's body some term must be added to the left-hand side of equation (A), which will alter the absolute term of equation (C) and the $\frac{1}{2}$ under the square root in equation (E) so as to increase W, and a number of small corrections of other kinds might be made, which, however, considering the roughness of the numerical data we have to go upon, are not worth taking into account.

The justification of the assumptions as to the smallness of a and s is best exhibited by a numerical example. Taking then AV $= 120$ with A $= 2$ square feet per pound of bird, and assuming S $= 0{\cdot}5$ (a stroke of 1 foot) we find that if $p = 30$ (about 300 downward strokes per minute, which sea-gulls in regular flight often exceed) and $\cos\theta = 1$, $\mathrm{S}p\cos\theta = 15$, and W $= 2{\cdot}65$; while μ is $0{\cdot}35$, and m is $0{\cdot}041$. The maximum value of a is then $0{\cdot}041(1+0{\cdot}35)$ when $pt = \pi$. This is $0{\cdot}0554$, the circular measure of 3° 10′ nearly. The maximum value of s, the slope of wing path, is $p\mathrm{S}/\mathrm{V} = 0{\cdot}25$; the circular measure of 14° 20′; but these do not occur simultaneously.

It will be found that their sum is a maximum when $pt = 0$ and is then $0{\cdot}28$, the circular measure of 16°; thus lying well below the limit of 20° suggested in the earlier part of this paper. The body-path, on integration, and taking $\mathrm{Z} = 0$ and $d\mathrm{Z}/dt = 0$ initially, is $\mathrm{Z} = -\mu g/p^2(1-\cos pt)$ and the amplitude is $0{\cdot}0125$ foot. The body, therefore, pursues a simple harmonic path, whose average level is about 5/32 inch below the mean level of the wing-path, and whose total rise and fall from crest to hollow is almost 5/16 inch. The relative motion of wing and body does not sensibly differ from that of the wing alone.

The investigation of the question as to hovering is far less satisfactory. We might assume, for instance, that the centre of pressure of the wings followed a circular or elliptical orbit, the plane of the wings making a small angle with the tangent to the orbit, and being variable as before, so as to maintain, on the whole, an upward pressure. If we make this angle $a = m\sin pt$, and form the equation $d^2\mathrm{Z}/dt^2 = g(\mathrm{F}-1)$

as before, remembering, however, that though a may be assumed small, the angle of slope, s, of the wing path, is not, we shall find, if the orbit be taken to be circular, $m = 1/kV^2$ where V is the velocity of the motion along the orbit. In order that m may have values similar to those in the case of progressive flight, the frequency of flapping must be much higher, twice as fast, or thereabouts. But there are several considerations which render results of this kind much more unsatisfactory than those previously obtained. In the first place the motion assumed is very much less like that of a bird's wing than in the former case, inasmuch as it involves complete revolution of the plane of the wing about a horizontal axis in its own plane, and, in the second place, the edges of the plane are continually cutting across the eddies, created by their previous motion, in a very different way from that in which they interfere with those created in progressive flight, so that the numerical value of k, and the value of the function of V which is put down as 1 in the formula $d^2Z/dt^2 = g(F - 1)$ are really unknown, and cannot be inferred from experiments with soaring planes, or even propellers. We can only expect that the actual numbers will be tolerably like those given by Langley's and other such experiments.

All that can be inferred, therefore, is that, provided a sufficiently high speed of flapping is attainable, we may reasonably anticipate that the horse-power for hovering need not differ very materially from that for progressive flight. Internal stresses in the wings or machinery, due to their inertia, as well as physiological difficulties connected with high velocities of reciprocation, may also come in to limit the rate of flapping, and prevent an equally economical rate of working being attainable in hovering, as in progressive flight. A considerable simplification of the mere mathematics could have been effected by at once assuming that the arrangement for altering the angle a was so contrived as to make the pressure P_a, or its vertical component, F, an assigned function of the time; but this has the objection that, there being then no expressly formulated relation between a and P or F, experimental evidence would be wanting to settle the values of the numerical constants involved. In fact, the more the mathematical work is generalized, the less definite do the numerical results become, in the present state of our experimental knowledge, and this must form the writer's apology for working out, in a mathematically clumsy way, so limited a case of a problem which has been treated already, in a much more powerful and general way, by far abler hands. For example, Thomson and Tait mention, as an example of motion of a solid in a liquid, the vibratory and irregular movements of such an object as an oyster shell sinking pretty slowly with a swaying motion when thrown flatwise into water. The rate of expenditure of energy necessary to prevent its sinking would, if these motions were forcibly produced,

evidently practically coincide with the rate at which gravity works, when the rate of sinking is, on an average, uniform, and should be quite small. The case of hovering flight of a bird is manifestly closely analogous, and that of progressive flight is similarly closely analogous to that of the same oyster shell, or a piece of slate, projected under water and forcibly maintained in a state of rhythmic motion. The only use of the investigation given in the present paper is to reduce the matter to figures, in a case sufficiently simple to enable us to use numbers supplied by existing experimental results.

[*Note added March* 21, 1899.—The error alluded to in the previous note was detected by checking equation (A) and that for H to see if the horizontal component of the acceleration was exactly periodic. Equation (C), as now given correctly, is the condition in question, with the additional information that each side is the work done. Mr. F. Purser, F.T.C.D., suggested that the wings as well as the body might be supposed heavy. The author has examined this point, and finds that, supposing, as before, that the engine, say a cylinder and piston, to whose rod the aëroplane is attached, works vertically, no difference is made in equation (C). The force P_a being inclined, has, or may have, a moment about the centre of inertia of the whole machine; the effects of this should be wholly periodic, but inasmuch as it appears from experiment and otherwise that P does not pass through the centre of figure of the plane, and moves about in some way depending on the angle α, it would be impossible to test this condition without assuming details of dimensions, &c. Any actual bird or flying machine, must have some steering apparatus, capable of correcting disturbances of a rotational kind in both the vertical and the horizontal plane, but its management plainly cannot involve any material alteration in the work to be done. The author conceives it to be possible, if not highly probable, that the motion in flying is, in respect of direction, unstable. Small periodic terms may be assumed also to be added to V, but lead to nothing up to the order of quantities involved in neglecting the difference between cosine and radius.]

"A Preliminary Note upon certain Organisms isolated from Cancer, and their Pathogenic Effects upon Animals." By H. G. PLIMMER, M.R.C.S., F.L.S., Pathologist, and Lecturer on Pathology and Bacteriology, St. Mary's Hospital, London. Communicated by Professor J. ROSE BRADFORD, F.R.S. Received February 22,—Read March 9, 1899.

(The following specimens were exhibited at the reading of the paper:—

1. Sections of the cancer from which the cultures were made.
2. The cultures on various media.
3. Preparations of the cultures.
4. Sections of the organs of the animals in which tumours have been produced.
5. Animals, or portions of them, in which tumours have been produced.)

During the past six years I have been studying the cell-inclusions found in cancer, and their relation both to the origin and course of the disease; and for this work I have had to examine 1278 cancers taken from various organs and parts, and of all possible varieties. Out of this large number of cases there have been a few—nine in all—in which the cell-inclusions have been extremely numerous; so that at the growing edge, and even far into the tumour, scarcely a cell could be found without an inclusion, sometimes with as many as thirty-six even of these inclusions in one cell; and these bodies have been similar to those which Metschnikoff, Ruffer, and others, as well as myself, have regarded and described as parasites, standing in causal relationship to the disease. In two out of the nine cases mentioned, these bodies have been present in enormous numbers; and I have succeeded in isolating from the last of these remarkable cases, an organism, which is pathogenic, in a peculiar manner, to certain animals, and whose virulence I have been able to keep unimpaired for some months.

Previous Work on the Experimental Production of Tumours in Animals.

The only work, I think, that needs mention here in connection with this heading is that of Sanfelice, in Cagliari, and of Roncali, in Rome. Sanfelice has produced tumours in animals with organisms which he isolated from infusions of various fruits; and they both have isolated organisms from cancers, which, I believe, from their descriptions—I have not seen their cultures—are morphologically somewhat similar to those I am about to bring before you. But Sanfelice's organism appears to have been very difficult to isolate in a virulent form from human cancer, and to keep virulent; so that in his last paper,* he

* 'Zeitschrift für Hygiene,' 1899.

treats only of the organisms derived from fruit infusions, and of their effects upon animals. Most of their statements are doubted by the German pathologists, including such a good observer as Baumgarten. But, from my own experience, I do not find any reason to doubt any of Sanfelice's statements; and I think that he deserves the greatest credit for removing the study of the ætiology of cancer from the histological to the experimental region of work.

On the Method of Isolation adopted.

The cancer, from which the organisms I describe were isolated, and with which my experiments have been made, was taken from the breast of a woman, aged thirty-five years; it had a history of only two months' duration, and it was growing rapidly at the time of the operation. Immediately after removal, I examined a fresh scraping, and, finding such an extraordinary number of the bodies I have mentioned in the cells, I cut, with all possible precautions against contamination, with a carefully sterilised knife, very thin slices from the growth, which I placed with a little of the juice scraped from the cut surface in a flask containing the following liquid, which was of course carefully sterilised. This medium consisted of an infusion made from cancer, just as the ordinary beef infusion is made, to which was added, after careful neutralisation, 2 per cent. of glucose and 1 per cent. of tartaric acid. This medium was the outcome of many trials with all kinds of mixtures, and I tried it in this case as I had already got similar organisms to grow in it from two previous cases; but they had no pathogenic properties, and this, I think, was due to the omission of the next step. This medium, too, is particularly useful, as hardly any bacteria, however hardy, will grow upon it.

Then, remembering that in the body these organisms were under anaërobic conditions, I exhausted the air from my flasks, and passed hydrogen into them, finally sealing them up. This I have found is of great importance as regards the maintenance of the virulence; and I find, consequently, that there is no falling off in the virulence of my cultures, which are as active now as they were four months ago. Five flasks were made in this way, but, in spite of precautions, two became contaminated with moulds; in the other three, however, I got, after from three to five days, a pure culture of the organism I describe, and which has been kept growing in this and various other media ever since.

Morphology and Relation to Media.

The organism is apparently a saccharomyces; but I am informed that, according to some authorities (such as De Bary, Cuboni, and

Duclaux), the Saccharomycetes are nothing but the developmental stages of fungi which really belong to either the Phyco-, Asco-, or Basidio-mycetes. Moreover, they state that in some species of mycelium-forming fungi, single parts, especially conidia, can grow in the saccharomyces form on certain nutrient media: so I will not attempt to locate this organism at present. Sanfelice and Roncali, however, definitely state that the organisms they have isolated are Blastomycetes.

When grown in the medium described, these organisms produce a cloudiness which becomes visible in about forty-eight hours, and increases till about the sixth day, when the growth sinks to the bottom, the medium then becoming clear; no scum or pellicle is formed.

When grown on this medium solidified with agar, the organisms form small round colonies which remain separate; after some weeks the colour, which was originally white, becomes yellow; the colonies do not attain a much greater size than those here shown.

Gelatin is not liquefied, but the growth on this medium is never luxuriant. On potato, a thick white layer is formed, which in about two weeks will cover the entire surface, changing then to a yellowish-brown colour.

They will grow aërobically, but not so well, at any rate at first; and they lose their virulence in a short time if continuously grown in this way.

Microscopically, they are round bodies, frequently growing in clumps, with a nucleus which stains deeply, and, in most cases, with a thin, strongly refractile capsule, which sometimes shows a double contour; but some young forms can be seen which are apparently without a capsule. The size varies from 0·004 mm. to 0·04 mm.

Their reproduction appears to be by budding, but I have fancied that I have also seen, in a few instances, endogenous budding; of this, however, I am not certain.

These bodies correspond morphologically with those found in the original tumour, and also with those described by Ruffer and myself, and by some others of those who have worked at the microscopical appearances of cancer.

Experimental Results.

I have selected, from the experimental work which I have done with these organisms, those experiments which seemed to me to be the most important. Up to the present, I have not been able to make any experiments upon such animals as would allow of the easy bringing of these organisms into contact with a likely epithelial surface, with the exception of the cornea (*vide* Experiment 4 below); but, through

the kindness of Dr. Bradford, I have been enabled now, at the Brown Institution, to inoculate a bitch in the mammæ, but the time is as yet too short to enable me to make any statement as to the result.

The cultures used in the following experiments were made in the medium previously described.

(1) *Rabbit.*—Intravenous injection of 5 c.c. of an eight days old culture.

No obvious result.

The rabbit was killed thirteen weeks after and found apparently normal.

(2) *Rabbit.*—Intraperitoneal injection of 10 c.c. of a twenty-one days old culture.

No obvious result.

The rabbit was killed eight weeks after and found apparently normal.

(3) *Rabbit.*—Subcutaneous injection of 5 c.c. of a three days old culture.

No obvious result.

This rabbit was used later for experiment No. 4, and when killed, fourteen weeks after this experiment, was found normal; nothing was found at the seat of injection.

(4) *Rabbit.*—Both corneæ were scraped, and the sediment of a ten days old culture rubbed over them.

The rabbit was killed in forty-eight hours.

There was considerable proliferation of the corneal epithelium, which had forced its way into the subjacent tissue.

The organisms were found in the epithelial cells, after fixing and staining, with appearances similar to those of the inclusions in cancer cells, as described by Ruffer and myself.

(5) *Rabbit.*—Trephined and inoculated beneath the dura mater with 5 c.c. of a seven days old culture.

The rabbit died in nine and a half days; wound healed.

The brain and cord contained the organisms in large numbers. Pure cultures were obtained from brain, liver, and kidney. Nothing obtained from blood, spleen, or peritoneal fluid.

(6) *Rabbit.*—Trephined and inoculated beneath the dura mater with 5 c.c. of a three days old culture, made from brain of No. 5.

The rabbit died in eight days; wound healed.

Organisms found in heart, blood, brain, and cord. Pure cultures made from brain, cord, kidney, and liver.

(7) *Guinea-pig.*—Subcutaneous injection of 5 c.c. of a ten days old culture.

No obvious result.

The guinea-pig was killed in fifteen days, and was found apparently normal; nothing was found at seat of injection.

(8) *Guinea-pig.*—Intraperitoneal injection of 10 c.c. of a three weeks old culture.

Died in twenty days.

Liver, lungs, and peritoneum studded with new growths of a white colour; lymphatic glands in abdomen enlarged. Pure cultures made from liver, lungs, and abdominal glands; nothing obtained from blood.

Sections of the above mentioned parts showed new growths of an endothelial nature, with the organisms within the cells, and free in the tissues.

(9) *Guinea-pig.*—Intraperitoneal injection of 10 c.c. of an eight days old culture, made from abdominal glands of No. 8.

The guinea-pig died in seventeen and a half days, and showed the same appearances as No. 8.

Cultures were made as before, and also from the blood. In this case the omentum was also studded with new growths.

I have given here some of the failures and successes which have been constant; and I should like to add that Professor Wright, of Netley, has repeated some of the experiments I have made, and that his results coincide with mine.

The important point, of course, of all this is—the experimental production of malignant tumours in animals by an organism isolated from a malignant tumour in man. That these experimental tumours are, so far, of endothelial origin is due to the fact that until I was enabled to inoculate a dog, I found it very difficult to get the organism in contact with likely epithelium; all the above methods of inoculation, save one, could only bring them into contact with endothelial surfaces. No. 4 (the corneal experiment) is the only one in which an epithelial surface was tried; and in this case the great proliferation of the epithelium, the appearances of the organisms in the cells, and the irritation produced, are very striking. But the fact of being able to excite a malignant growth with an organism isolated from cancer is, I think, a point of some importance in the ætiology of cancer.

I am at present experimenting with the view of observing the effects produced by these organisms when brought into contact with epithelia.

The deductions which I think may fairly be made from these observations and experiments are as follows:—

(1) That there are certain cancers, which occur very rarely, in which there are, in enormous numbers, intracellular bodies of the kind described by Ruffer, myself, and others, as parasitic Protozoa. (From the rarity of these cases and their comparatively acute course, one is tempted to think that they are not due to the same cause as ordinary cancers; but there is really no more difference between them and ordinary cancers than between acute and chronic tubercle.)

(2) That by the use of appropriate means these intracellular bodies can be isolated and cultivated outside the body.

(3) That these cultures, when introduced into certain animals, can cause death, with the production of tumours, so far of endothelial origin; and that pure cultures can be made from these growths which, when inoculated into suitable animals, will produce similar tumours.

"On the Gastric Gland of Mollusca and Decapod Crustacea: its Structure and Functions." By C. A. MACMUNN, M.A., M.D. Communicated by Dr. M. FOSTER, Sec. R.S. Received February 23,—Read March 9, 1899.

(Abstract.)

In 1883 I communicated a paper* to the Royal Society, in which I described the occurrence of a pigment closely resembling vegetable chlorophyll in the so-called liver of Invertebrates, and in 1885 a further contribution in continuation of the same subject, which was published in the 'Philosophical Transactions' (Part I, 1886).

I named this colouring matter "enterochlorophyll," because after comparing it with all the animal pigments and plant pigments known to me, it seemed to resemble, both in its chemical and spectroscopic characters, the chlorophyll of plants.

In the latter paper, I endeavoured to describe the microscopic characters of this pigment, as it was found in the digestive gland, and I applied all the tests then considered to be distinctive of chlorophyll to the solutions of the pigment. I found that whereas enterochlorophyll appeared to be a chlorophyll, or a modified chlorophyll, it yet differed in some respects from chlorophyll, as it is obtained *directly* from fresh green leaves. Some recent writers have called in question the right to call this pigment by the above name, so I have reinvestigated the whole subject.

It was, however, necessary first of all to study the histology of the digestive gland, or gastric gland, as it is now named, and the microscopic characters of the pigment found in it. This has been done by Max Weber and Frenzel for the gland of crustacea, and by Barfurth and by Frenzel for the gland of mollusca.†

As can be gleaned from these observers, great difficulties attend the preparation of this gland for microscopic observation. I found after numerous failures that formol is the best fixative, used in stronger solution than it usually is employed in vertebrate histology. Thus it is necessary to employ solutions containing from 20 to 30 per cent. of

* 'Roy. Soc. Proc.,' vol. 35 (1883), p. 370.

† References are given in the complete paper.

formol. After from 12 to 24 hours the preparation is transferred to alcohol of 95 per cent., and, when hard enough, to a mixture of alcohol and ether, and finally into a solution of celloidin. When this is set, the preparation is cut either in the ordinary way, or by the freezing microtome. Clearing is done by means of oil of sandalwood, or oil of origanum. The sections were stained in various ways, but the best results are obtained with hæmalum, followed by eosin as a plasma stain. I also used mucicarmine, thionin, "Soudan III," and other stains for special purposes. In this way very satisfactory preparations can be obtained, and the celloidin keeps all the gland constituents *in situ.*

The glandular epithelium in the crustacea contains, according to Max Weber, two kinds of cells, hepatic cells and ferment cells. Frenzel subsequently called them fat-cells, or fat-holding cells, and ferment-cells. That of mollusca contains, according to Barfurth, hepatic cells, and ferment cells also, which Frenzel again re-named granular cells and club-cells. Now the sharp distinction drawn between these kinds of cells by these observers is found not really to exist, and we meet with transition-forms between them. The coloured contents of these cells, in the case of either ferment-cells or granular-cells, or fat-cells, are coloured by either enterochlorophyll or by a lipochrome, or by both.

It was, however, mainly my object, after having studied the histology of the gastric gland, to find out if I was really justified in calling this colouring matter enterochlorophyll, and if it has properties which class it among the chlorophylls, to find out how it gets into the gland, and so on.

It is impossible here to refer more fully to the histology of the gland, so that I shall merely give an account of the observations on the pigments found in it.

The only way in which any pigment giving a banded absorption-spectrum can be readily identified is, of course, by means of the spectroscope, but since spectrum analysis alone is attended by certain fallacies, one has to fall back on spectrophotometry. This being a quantitative method gives the most accurate results of all. Of course elementary analysis, if applicable, enables one at once to decide the identity or non-identity of pigments, but unfortunately the pigments with which we are now dealing cannot be prepared in a pure, or even in an approximately pure, condition for this purpose with our present knowledge, owing to their being mixed with fatty matters and other impurities.

The spectrophotometer which I have used is a modified form of that which Vierordt introduced. Some of the modifications were, I believe, introduced by the brothers Krüss;* others were suggested by me to Mr. Hilger, who constructed the apparatus.

* G. and H. Krüss, 'Kolorimetrie und Quantitative Spektralanalyse,' 1891.

When a solution of plant-chlorophyll in alcohol solution is compared with a similar solution of enterochlorophyll by means of curves obtained with the spectrophotometer, these curves do not correspond; but when we convert the plant-chlorophyll into the "modified" form, or, what is the same thing, the slightly acid form, by means of acetic acid, and allowing the solution to stand for a few hours, and then compare the respective solutions, we find the maxima and minima of the curves follow each other so closely as to lead one to conclude that the pigments are closely related to each other.

Again, if hydrochloric acid is added to a solution of plant chlorophyll in alcohol, and to a solution of enterochlorophyll in alcohol, and these two solutions are examined by means of the spectrophotometer, a remarkable agreement is noticed.

The numbers taken for these measurements are the percentages of the unabsorbed light, as the latter enable curves to be more easily constructed than by taking the co-efficients of extinction.

I have also examined Lankester's "chætopterin,"* and I find the curve obtained by the spectrophotometer follows closely that of enterochlorophyll and that of modified chlorophyll. But chætopterin is soluble in glycerin, while enterochlorophyll is not.

While examining *Chætopterus* at the Plymouth Laboratory, I found that an alcohol solution of the contents of the intestine, in the neighbourhood of that part of the gut coloured by chætopterin, gave exactly the same spectrum as a similar solution of chætopterin itself. The discussion of the inferences to be drawn from this observation may, however, be left for the present.

I have seen enterochlorophyll present in a finely granular form in the intestinal epithelium of *Patella*, and in the pseudo-villi of the glandular stomach of the same mollusc one can see crowds of leucocytes, some of which are insinuating themselves between the columnar epithelial cells. The inference, of course, is that the leucocytes carry away those substances which have been taken up by the epithelial cells in a more or less digested condition. Some have supposed that these granules are being *excreted* into the lumen of the gut, but in my opinion, based upon a study of numerous sections of invertebrate gastric glands, the excretion of enterochlorophyll by means of the gland cells—belonging to the various kinds mentioned—*takes place into the lumen of the alveoli, acini, or tubes of the gastric gland, and from these we can trace the excreted gland-cells into the intestine.*

From all these and other observations I have been forced, I must confess against my inclination, to believe that enterochlorophyll is a pigment which primarily has been taken up from the intestine *dissolved in a fatty medium*, and is carried either by leucocytes, or in some other way to be deposited with this fat, and perhaps other reserve

* 'Quart. Journ. Micros. Sci.,' vol. 40, p. 447, &c.

products, in the gastric gland. Whether it is utilised for the production of other pigments or not is a question for future investigation. That it is a chlorophyll derivative I now believe to be proved. Its stability, as compared with plant chlorophyll, is due to the fact that it has been altered by the action of the digestive juices. Such derivatives of complex mother-substances are, as is well known, much more stable, and less prone to change than the parent pigments.

"On the Structure and Affinities of *Matonia pectinata*, R. Br., with an Account of the Geological History of the Matonineæ." By A. C. SEWARD, F.R.S., University Lecturer in Botany, Cambridge. Received February 28,—Read March 9, 1899.

(Abstract.)

The genus *Matonia* has long been known as an isolated type among existing ferns. It is represented by two species, *M. pectinata* R. Brown and *M. sarmentosa* Baker, both confined to the Malayan region. Matonia has not hitherto been examined anatomically, and its reference by several writers to an intermediate position between the Cyatheaceæ and Gleicheniaceæ, is based on the structure of the sorus, which, in the small numbers of sporangia and in its circular form, resembles the latter family, while the presence of an indusium and the position of the annulus afford connecting links with Cyatheaceous ferns.

In *Matonia pectinata* the frond has a characteristic pedate habit, with numerous long pinnæ having slightly falcate linear segments, practically all of which appear to be fertile. The sori are circular in form and indusiate, consisting of about eight large sporangia with an oblique incomplete annulus, containing sixty-four tetrahedral spores. The dichotomously branched rhizome, which grows on the surface of the ground, is thickly covered with a felt of multicellular hairs, and gives rise to long-stalked fronds from its upper face, and a few wiry roots, which may arise from any part of the surface of the stem.

The full paper deals more especially with the anatomical structure of *Matonia pectinata*. The material which rendered the investigation possible was generously supplied by Mr. Shelford, of the Sarawak Museum, Borneo, to whom the author wishes to express his hearty thanks.

The stem is polystelic, and of the gamostelic type; there may be two annular steles, with the centre of the stem occupied by ground-tissue, or in shorter branches of the rhizome a third vascular strand may occupy the axial region. Each stele consists of xylem tracheids and associated parenchyma, surrounded by phloem composed of large sieve-tubes, with numerous sieve-plates on the lateral walls, and phloem

parenchyma; an endodermis and pericycle surround each stele, and in the case of the annular steles these layers occur both internally and externally. At the nodes the outer annular stele bends up into the leaf-stalk, and a branch is also given off from the margin of a gap formed in the inner annular stele; the axial vascular strand may or may not be in continuity with the meristele of the leaf. The petiole is traversed by a single stele, similar in shape to that of certain Cyatheaceous ferns; towards the top of the leaf-stalk the stele alters its form, and gradually gives off separate U-shaped branches to supply the pinnæ.

The most interesting feature in the structure of the pinnules is the marked papillose form of the lower epidermal cells. The roots have a triarch stele enclosed by a few layers of thick brown sclerous cells.

In structure *Matonia pectinata* presents points of agreement with several families of ferns, on the whole approximating more closely to Cyatheaceæ than to any other family; but the peculiarities are such as to fully confirm the conclusion previously drawn from external characters that Matonia should be placed in a separate division of the Filices.

After comparing the structure of the Malayan species with that of other fern genera, the paper concludes with an attempt to give an account of the geological history of the Matonineæ. The genera *Laccopteris* and *Matonidium* are dealt with at some length, and reference is made to other Mesozoic ferns, which may probably be included in the same group.

The data furnished by an examination of palæontological evidence lead to the conclusion that in Matonia we have a survival of a family of ferns, now confined to a few localities in Borneo and the Malay peninsula, and represented by two living species, which in the Mesozoic epoch had a wide geographical range, being especially abundant in the European area.

"Note on a new Form of light Plane Mirrors." By A. MALLOCK. Communicated by LORD RAYLEIGH, F.R.S. Received February 28,—Read March 9, 1899.

Having recently, in the course of some experiments on air waves, had occasion to make use of some very thin films as coverings for the openings of resonators, it occurred to me that such films, if stretched over rings with edges ground to a true plane, might be used as plane mirrors, and the following note records the results of the trials made with this object in view.

The best films were obtained by letting a few drops of a solution of pyroxyline in amyl acetate spread on the surface of water and lifting

the film, left after the evaporation of the solvent, on a ring previously placed beneath the surface.

Good films can also be obtained in this way from copal varnish, but they are more difficult to handle, and the intrinsic strength of the material is not nearly as great as that of pyroxyline.

In order to lift the films off the surface of the water without folds, and without getting any of the parts doubled, it is desirable that the ring used for the purpose should be not less than half an inch deep, *i.e.*, a length of half an inch cut off a tube of the required diameter, or a strip half an inch wide bent into a circle and the ends joined. If the depth is less than this the overlapping part of the film is liable to be carried round the lower edge by the capillary action of the water, and to spread itself irregularly on the under surface of the stretched part while being lifted.

The ring should be rested on a wire tripod with long turned-in feet so that the pyroxiline solution may spread well beyond its edges before encountering the tripod legs; and, in order that the film while being lifted may not be broken by the drag of the water enclosed by it and the ring, air should be introduced inside the latter through a pipette during the process. A little practice makes it quite easy to secure uniform films with certainty in this way.

After removal from the water the film should be allowed to drain for some minutes until the surface is free from drops, and it may then be stretched over the worked ring whose edge is to form the boundary of the mirror. In order to do this the worked ring is placed on a circular support some inches in height and a little larger in diameter than the ring itself. The film, which at this stage of drying is tough and extensible, is then laid in position over it, and the lifting ring forced downwards until the film tears away. It will be then found that the film is left well stretched over the worked ring and adherent to its edge and side.

The lifting rings should be about half an inch larger in diameter than the worked rings.

If the film is not much more than a twenty-thousandth of an inch in thickness its surface is glassy and reflects light well, but if much thicker than this it tends to show a crape-like or finely grained structure, and its quality as a reflector is not so good.

I found at first great difficulty in silvering the films: in fact as the film comes from the water, it refuses altogether to receive the silver deposit, but if well washed under a very gentle stream of distilled water as soon as it is stretched on the mirror ring, and then thoroughly dried, a fair coating of silver may be obtained.

It seems essential that the amyl acetate should be completely evaporated, and this being accomplished, the other requisite is absolute surface cleanliness both of the film and the silvering bath.

To ensure this requires considerable care. The film after being washed should be dried as quickly as may be in a warm drying chamber, and it is advisable not even to touch the ring with the hand until its sides are quite dry. The dish also in which the silvering solution is placed must be carefully washed just before use, first with nitric acid, then with water, and finally with distilled water; but on no account should it be wiped or touched with the hand.

Before taking these precautions, I never succeeded in getting even a tolerable silver surface, though I tried many other devices which need not now be described. Even the best results I have hitherto obtained are not quite satisfactory, so very ready are the films to lay hold of any kind of scum with which they may be brought in contact.

The best material for the mirror rings I found to be glass, which is much more easily ground to a true surface than most of the metals I tried, and suitable rings of very thin glass may be easily obtained by cutting up beakers or test-tubes with a glazier's diamond in a lathe.

The edges may then be quickly ground to a plane on a flat lap with coarse emery or carborundum (which I prefer), and finished with fine emery, though as the edges need not be polished it is not necessary to use nearly such fine emery as is required for ordinary optical surfaces.

The most important point in getting the edge of the ring to a true plane, is to use a very light pressure in grinding, and the thinner and weaker the ring the lighter the pressure required.

When the grinding is well done, the definition given by reflection from the stretched film is quite equal to that of a worked glass surface of the same area, at any rate up to 2 inches or 2½ inches diameter, and up to this diameter also it makes no preceptible difference to a reflected pencil of rays whether the plane of the mirror is vertical or horizontal, showing that the weight of the film has no appreciable effect in determining its form.

The weight of these mirrors is practically the weight of their supporting rings, and with reasonable care a 2-inch ring may be made weighing considerably less than 10 grains, and although it will probably be impossible to rival with them the brilliancy of silvered glass, there are I should think many cases where good definition, and extreme lightness are requisite (as for instance in the suspended mirrors of photographic recording instruments) in which such mirrors as these might be useful.

March 16, 1899.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The CROONIAN LECTURE, "On the Relation of Motion in Animals and Plants to the Electrical Phenomena which are associated with it," was delivered by Professor BURDON SANDERSON, F.R.S.

The following Papers were read:—

I. "Experiments in Micro-metallurgy: Effects of Strain." By Professor EWING, F.R.S., and W. ROSENHAIN.

II. "On Transmission of Proteosoma to Birds by the Mosquito: a Report to the Malaria Committee of the Royal Society." By Dr. C. W. DANIELS.

The Society adjourned over the Easter Recess to Thursday, April 20th.

"On Transmission of Proteosoma to Birds by the Mosquito: a Report to the Malaria Committee of the Royal Society." By Dr. C. W. DANIELS. Communicated by Dr. M. FOSTER, Sec.R.S., by direction of the Malaria Committee. Received February 13,—Read March 16, 1899.

I have the honour to report the results of my observations since my arrival here (Calcutta) on December 21, 1898.

2. Major Ronald Ross, I.M.S., after demonstrating and explaining to me his method of dissecting the mosquito, showed me in prepared specimens the pigmented bodies met with in the stomach walls of mosquitoes fed on birds infected with Proteosoma, and also the changes which these bodies undergo day by day. Finally he demonstrated to me the "germinal threads" in cysts in the stomach wall, in the fluids of the body, and in the cells of the veneno-salivary glands.

3. On my arrival there were in the laboratory, in test-tubes, several series of mosquitoes which had fed on birds infected with Proteosoma on the nights of November 30, December 10, December 12, *December 15,* and December 20.

Of each of these series Major Ross dissected specimens, and demonstrated in them the same bodies that he had already shown me in prepared specimens. He pointed out that in the older mosquitoes it was possible to predict from an examination of the fluid obtained on cutting the thorax the nature of the contents both of the "coccidia" (the term employed by Ross)* in the stomach, and of the cells of the veneno-salivary glands.

These points I readily observed.

4. Of the mosquitoes referred to I day by day examined those which died, and others which I killed. In these I was able to repeat the observations and, in insects belonging to the earlier series, to trace the changes in the size and in the nature of the contents of the "coccidia."

I also examined a large number of mosquitoes caught about the laboratory, and others which had been raised from larvæ. In none of these did I find either "coccidia" in the stomach wall, germinal threads in the body fluids, or germinal threads in the cells in the salivary glands; nor did I find "black spores" (Ross).

5. Major Ross informed me that his published results were based on observations made in the hot season, when the temperature was 80° F., or over; and that now, as it was the cool season, I should find the changes progress more slowly, although the sequence of events was the same. My observations on the mosquitoes fed on December 20 and December 15 showed that this was the case. Major Ross also informed me that, with the lowered temperature, mosquitoes fed less readily, and that more difficulty was experienced in rearing them to a spore-bearing age.

These difficulties the use of the incubator was only partially successful in overcoming.

6. On the evening of January 1, following exactly in Major Ross's lines, I commenced a repetition of his main experiment:—

A large number of grey mosquitoes, reared from larvæ, were released in two mosquito nets.

In net No. 1 four birds were placed. On December 31 I had already found Proteosomata in large numbers in three of these birds, and in the fourth in moderate numbers.

In net No. 2 two birds, in whose blood no Proteosomata had been found, were placed. These two birds died two and three weeks later; on dissection no black pigment was found in their organs. Repeated examinations of their blood had failed to discover Proteosomata.

On January 2 none of the mosquitoes had fed, and on January 3 only two in net No. 1 and eight in net No. 2. On January 4, *which* was a warm night with a minimum temperature of 59·2° F., *sixty-three* mosquitoes were found in the morning gorged with blood

* See note by the Malaria Committee appended to this Report.

in net No. 1, and were caught in separate test-tubes which were then plugged with wool and placed in the incubator. Of the control series in net No. 2, where the non-infected birds had been placed, eighteen were caught and treated in the same way.

On the following two evenings, with minimum temperatures of 60·7° and 63·2°, sixty-three and forty-six mosquitoes were fed on the infected birds, net No. 1, and were kept for the preparation of specimens; and twelve mosquitoes were fed on the non-infected birds, net No. 2, bringing the number of the control series up to thirty-eight. At a later date eighteen mosquitoes were fed on a blue jay with numerous halteridia.

On the third day the sixty-three mosquitoes, from net No. 1 (with exception of those previously killed for examination or which had died) were released inside a clean net free from other mosquitoes. Birds free from Proteosoma were also placed in this net.*

In the morning all mosquitoes found inside were collected. Most of them had fed well. The minimum temperature during the night had been 63·2° F.

The mosquitoes were not fed on the following night as they were full of blood which most of them voided during the night. Many died next day.

The remainder were given the opportunity of refeeding every night after this; but as a spell of cold weather set in with minimum temperature of 44—49° F. (only on one night did it exceed 50° F.) few fed well or at all, and there was a consequent continued heavy mortality. Only one insect, which subsequently escaped in the night, being alive on the 10th day.

This method of feeding was very unsatisfactory in exceptionally cold weather. During the day the mosquitoes being kept warm in the incubator rapidly digested their food, whilst at night the cold rendered them torpid and they did not feed.

The control mosquitoes, of net No. 2, were treated in exactly the same manner, being fed on birds free from Proteosoma. The last died on the 13th day.

7. The results of the two series are as follows:—

Of sixty-three mosquitoes fed on proteosomal birds, forty-nine were

* This is the method Ross employs to re-feed mosquitoes. If infected birds are employed to re-feed the insects, a younger generation of coccidia is produced; I therefore used sterile birds for this purpose.

The method works fairly well in warm weather, but there is always some loss, as the full number is never collected again in the morning. As the process is repeated over and over again, this loss becomes serious, the more so the longer the period required for maturation of the coccidia. Moreover, in a frequently repeated process of this kind there is always the possibility of an outside mosquito getting *inside the net,* and to that extent vitiating the experiment.

examined, three were reserved for sections, one was too much decomposed for satisfactory examination; ten were not accounted for, having been lost in the nets.

Of the forty-nine examined two were killed on the first day,—that is, under twenty-four hours, and possibly under twelve hours after they had fed; no coccidia were found in these. Two more were examined the following morning, that is under thirty-six and possibly under twenty-four hours after they had fed; no coccidia were found in these.

In two examined about 4 p.m. of the same day, that is, under forty-six and possibly not more than thirty-four hours after they had fed on the infected birds, minute pigmented coccidia were found.

The remainder were examined on the following days. The largest numbers (eighteen) were examined on the fourth and (twelve) on the seventh days, as on these two days the mortality amounted to this.

In all these mosquitoes, with one exception, coccidia were found—usually in numbers; in one there was only one coccidium.

The exception occurred on the ninth day; but as by that time the insects had been re-fed several times, the mosquito in question may have been an outside one which had effected an entrance.

Of forty-five mosquitoes fed on the infected birds and examined, more than thirty-four hours after, forty-four contained coccidia.

This I may say is a more successful result than in the other series I have seen.

The other two series of mosquitoes were used by all of us for the preparation of specimens, and no record was kept of the number of non-infected insects. Judging from my own examination, only about three-quarters of them developed coccidia. Their treatment had been somewhat different, as for several days half of them were not incubated.

Of the controls fed on birds free from Proteosoma, thirty-eight in number, and treated in the same manner, twenty-nine were examined and nine are unaccounted for—lost in the nets. None of the twenty-nine were examined on the first day, but one was on the afternoon of the second day. The largest number, seven and five, were examined on what would correspond to the fourth and seventh days, four were examined on the fifth and four on the sixth days.* In none of these twenty-nine were coccidia found.

Of the eighteen fed on the blue jay with halteridia, twelve were examined from two to six days after feeding; none contained coccidia.

* It will be observed that these control mosquitoes were not, as the other series, collected on one, but on three nights. A very slight difference in breeze and light seems to affect the numbers that feed; any extra restlessness on the part of the birds *has the same result.*

8. The coccidia (pigmented bodies) found on the second day measured 6—7 μ, some of them a little more. They were oval bodies containing scattered granules of black pigment, and had a sharp, clear outline.

I incised the stomach of infected mosquitoes and by repeated washing and compression with a cover glass was able not only to wash out the contents, but even to express the loosely attached epithelium, so as to leave the stomach a transparent clear bag. The majority of coccidia remained fixed to the outer wall, though in one of the mosquitoes I observed a few coccidia escape with the epthelium. On subsequent attempts to detach the coccidia by this process I failed to do so, though some coccidia would be ruptured.

The next morning the smallest coccidia measured 10 μ; some were 12 μ. On the sixth day they were met with up to 30 μ; by this time the pigment had absolutely as well as relatively diminished.

In another three days some of them reached 60 μ; and in the last of the series examined (tenth day), there were coccidia measuring 70 μ.

The coccidia could now be seen to project from the outer wall of the stomach; very few contained pigment, and that only in small amount.

Some of the coccidia were clear, and others had a granular appearance; but in none were either black spores or germinal threads to be seen.

9. For the observation of the further development of the coccidium the early deaths of the mosquitoes, owing to the inclemency of the weather, rendered this series useless.

One of the insects infected on the night of January 5, and another infected on January 7, did reach this more advanced stage; and in the last of those fed on January 5, and which died on January 22, ruptured cysts, as well as numerous cysts containing mature germinal threads were found by me in the stomach wall, these threads were also found in the body fluids and in cells in the salivary glands. In one of the mosquitoes infected on January 5, which died on January 19, the coccidia had an appearance of striation.

In consequence of the effects of the unfavourable climatic conditions on the experimental insects, my observations on the development of the proteosomal coccidium were mainly made on mosquitoes infected November 30 and subsequent dates before my arrival, and on some infected on December 22.

On adding salt solution (15 grs. to the ounce) to an ordinary slide containing an infected mosquito stomach, and pressing on the cover glass, a projecting coccidium was ruptured; the contents poured out into the fluid, leaving the cyst wall still attached to the stomach.

The contents were seen to consist of a mass of shrivelled threads.

This appearance I frequently observed in the other series of infected insects already mentioned.

These threads, Ross's germinal threads, are sickle-shaped bodies, about 14 or 15 μ in length. They stain with logwood or methyl blue, but not strongly. On adding water or Farrant's solution they lose their shrivelled appearance, and become more rounded. Nearer one end than the other is an unstained portion (? nucleus). They show no signs of movement; but as they are invisible in water, and only become visible when shrivelled by the salt or stained, it may be doubted if they have been seen alive.

If the thorax of the mosquito at a somewhat more advanced stage in the development of the proteosomal coccidium is incised, similar threads will be found in the fluid exuded, if salt solution is added. In this case ruptured cysts can be found in the stomach wall.

The relation of the infection to the veneno-salivary gland involves a difficulty not met with in any other part of the examination.

The dissection of the stomach is easy; that of the salivary gland in its entirety is not, and for some reason appears to be more difficult in the old infected mosquitoes. Any rough manipulation results in the detachment of the cells, and little more than the duct is left. In most cases, however, even in old infected mosquitoes, one entire gland, or portions of both, can be exposed in fair condition.

In every case where this was done, and in which germinal threads were found in the body-fluids, the germinal threads were also found in some of the cells of the salivary gland. I failed to find similar threads in the large number of salivary glands obtained from uninfected mosquitoes bred from larvæ, or caught about the laboratory, or from mosquitoes at the earlier stages of proteosomal infection.

The affected cells, as they have a granular appearance, can be distinguished with a low power; the unaffected cells are quite clear.

With a high power, if not very numerous, the isolated germinal threads can be clearly distinguished in the cells; they are recognised by their peculiar shape and shrivelled appearance (the examination must be made in salt solution). If numerous, the individual threads can be better made out in the cells of the salivary gland than in the coccidia of the stomach wall; but, as in the case of the latter, pressure on the cover glass will rupture the cell, and the germinal threads are then poured out.

The threads do not fill the cell. There is a faintly granular crescentic portion on the side most remote from the duct which, in many cases at least, is free from threads. The part of the cell in which the threads lie must be nearly fluid, as it permits oscillation of the threads to take place.

The whole of the veneno-salivary gland is never involved. In one dissection made by Ross the cells in both middle lobes and in no other

part of the gland contained the threads. In several instances, where one gland has been exposed entire, the middle lobe alone has been involved; but in the majority all that can be stated with certainty is that the cells in one portion of the gland contain threads, and that those in other portions do not.

On these points I have satisfied myself by repeated examination, though the appearances are by no means difficult to make out.

I have gone at some length into the description of this matter, as, so far, we have found no satisfactory method of making permanent preparations. All the preservatives at our disposal, with the exception to some extent of weak formalin solution, wrinkle up the delicate cells; and I have no confidence in this agent as a means of making permanent specimens.

The following specific observations made by myself on mosquitoes dissected by Major Ross, Dr. Rivenberg, of the American Mission, who is working with Dr. Ross, and myself may be of interest:—

(*a*) Coccidial cysts full of apparently mature germinal threads; no ruptured cysts; no germinal threads in the body-fluids or salivary glands. Two observations.

(*b*) Cysts full of germinal threads; other ruptured empty cysts; germinal threads in body-fluids; germinal threads in salivary glands. Over twenty observations.

(*c*) Empty cysts in stomach wall; germinal threads in body-fluids of thorax; germinal threads in salivary glands; no cysts still containing germinal threads. Two observations.

(*d*) Empty cysts only in stomach wall; no germinal threads in body cavity; no germinal threads in well exposed salivary glands. One observation; the mosquito had been infected four weeks before death.

These observations fully confirm Ross's statement in every point. They indicate that the threads are formed in the coccidia; and that the germinal threads escape into the body cavity on the rupture of the coccidia, to be again collected in the salivary glands.

I should have liked to extend the series, but the continued cold weather renders it improbable that I shall be able to do so before I leave.

10. The infection of birds free from Proteosoma by the bites of mosquitoes.

On December 20, the day before my arrival, twenty-two birds were examined and found free from Proteosoma. On that night some of these birds were used for feeding the mosquitoes which had been infected on November 30 (?) and on the 24th and subsequent days; the remainder of the birds were used for feeding the mosquitoes first infected on November 30 and December 10, 12, and 15. In other

mosquitoes of this series germinal threads were found in the salivary glands; and those which fed, when examined later, gave the results indicated in paragraph 9.

On December 30 Dr. Rivenberg and myself examined these birds; three of them had Proteosoma, two in large numbers.

On January 4 I examined them all except one which died on January 2; in this bird the heart's blood contained no Proteosomata, and the organs were free from pigment.

Five more of them had now Proteosoma; in every instance the parasites were very numerous. On January 6 and 7 I again examined them; three more had Proteosoma, also in large numbers.

On January 9 no more cases had developed; but on January 18 one of the birds had numerous Proteosomata. It was also ascertained that many of these birds which previously had been found to be infected had now recovered, whilst others showed but a few Proteosomata.

Thus twelve out of twenty-two birds (54 per cent.) became infected. This compares unfavourably with Ross's earlier results, as, in his published series, twenty-two out of twenty-eight (79 per cent.) were infected. But it is to be remembered that at the time this result was obtained the germinal threads were found at the end of a week; whilst in December the development was much slower, and took at least twice the time. It is much easier to keep mosquitoes alive during the first week after feeding them than it is to keep them alive for any subsequent period; moreover, in hot weather, such as Ross had worked in, mosquitoes bite more readily.

These results appear less unfavourable, if they are considered in connection with observations on the normal proportion of wild, uncaged birds, infected with Proteosoma at this season. Thus, earlier in the year, Ross, out of 111 wild birds, found Proteosoma in fifteen, or 13·5 per cent.; whilst I found at this season only one out of thirty, or 3·3 per cent. affected with Proteosoma.

It is possible that in the cold season the birds have a greater power of resistance; the validity of this conjecture is rendered more probable by the short duration of the proteosomal attack in my infected birds. Of the twelve, five died within the first week. In three of the survivors, in which the Proteosomata had been very numerous, no parasites could be found ten days after the commencement of the invasion; in one in which they were never numerous none could be found on the fifth day. In the other three very few are now found, though at first they were numerous.

The recovery of these birds and the death of the mosquitoes fed on them diminishes the chances of much future work on this line during the time remaining to me here.

11. Mention has been made of the differentiation of the coccidia *(previous to* the formation of the germinal threads), according to the

appearance of their contents, into clear and granular; the evolution of the latter into the coccidia containing germinal threads can be traced day by day. This differentiation was clearly visible in my series.

In a minority of the coccidia, and in most infected mosquitoes, when the germinal threads are mature, certain black tubular bodies are to be found in cysts with otherwise clear contents. These black tubular bodies were frequently met with in the series of mosquitoes infected in November and December. Most of these mosquitoes contained some coccidia with black tubular spore-like bodies; though in a few insects all the cysts contained germinal threads only. In some cysts the black spores were numerous, and occupied the entire cyst; in other cysts there were only a few. In most instances germinal threads were not found in the black spore-bearing cysts; but there were a few such cysts in which it was doubtful whether germinal threads were present or not, or whether the appearance arose from over-lying threads which had escaped from a neighbouring capsule.

These black spores are very resistant; I have seen some which had been kept in water for months by Ross, and which had undergone no visible change. They withstand irrigation with liquor potassæ.

When the cysts are ruptured the black spores are to be found all over the body of the mosquito, but not included in cells. They do not seem to accumulate in any particular organ.

The most plausible view of the nature of these black spores seems to be that held by Major Ross, viz., that they are "resting spores," and that through them, by another cycle, the Proteosoma can be propagated in conditions unfavourable for direct propagation by mosquito-insertion into a warm-blooded animal.

If this be the case, three courses suggest themselves :—

(*a*) From the black spores may arise bodies capable of non-parasitic life (and possibly of reproduction), which at certain stages of their existence, and in certain conditions, on introduction into a warm-blooded host by inhalation, through drinking water, or even by injection by a mosquito or other blood-sucker in transferring them from the medium in which they live, may resume parasitic habits.

(*b*) That they may be ingested by mosquito larvæ, and in them undergo such development as will result in the formation of germinal threads in the adult mosquito, which, in turn, may be injected into the appropriate bird.

(*c*) That they may, if swallowed or inhaled by an appropriate warm-blooded host, so develop as to reach the circulation and pass into the sporulating phase.

Such experiments as have been made on this subject are inconclusive; and it is *obvious* that until the nature of these "black spores" is

determined we cannot exclude, even for Proteosoma of sparrows, the possibility of any one of the many possible alternative channels of infection. Intervention of the mosquito intermediate host may be only an occasional requirement.

Still less are we justified in concluding that malaria in man can only be acquired through and directly from the mosquito; or in devoting our attention exclusively to that channel.

12. I have made myself familiar with the Proteosoma in sparrows, and the Halteridium in pigeons and crows.

In one specimen of a "blue jay," also, I found a very abundant Halteridium infection; the parasites in this instance had some peculiarities which I hope to work out if we can procure more of these birds. The bird I had died before I had completed my observation; I have preserved the organs as well as specimens of the blood in the heart.

13. In the cardiac blood of this jay there were numerous filariæ. They were sheathless, sharp tailed and fairly active, and had locomotory movement. They were of two sizes; in the shorter the tapering of the tail was much more abrupt than in the longer. Neither showed any extension or contraction.

Adults of one species only, three females and five males, were found in the subcuticular connective tissue, and in that round the trachea.

They were much longer and thicker than *Filariæ clava* (Wedl) or than the filaria described by Mazzini in the pigeon.

The females have the usual double ovary terminating in a vagina which appears tubular near the vulva situated near the caudal end of the body. The mouth is terminal and unarmed; the anus is subterminal.

The male has two spicules of equal length. The thickness of these worms, and the fact that when placed in weak formalin (2 per cent.) the cuticle burst in its entire length, will make them suitable for determining some of the disputed points in the anatomy of the Filaridæ.*

14. The difficulties in connection with human malaria are increased by the present plague scare. The suspicion of the natives about inoculation, makes them averse to any intercourse with European medical men.

By rewards however we have been able to get two fair cases of tertian fever, and three cases with crescent plasmodia—two of them with crescents in considerable numbers. On these cases we have fed mosquitoes—the common grey, and two varieties of "dapple wings"

* Judging from the description of the embryos, it is probable that these blood-worms of the Indian blue jay are identical with those found by Manson in Amoy, China, in the magpie (*Pica media*) and the gray mina (*Gracupica nigricollis*), in which case the mature form of one will be found to lie in the pockets of the aortic and pulmonary semi-lunar valves (*vide* 'Journ. of the Queckett Micro. Club,' *vol. 6, p. 130,* No. 44, August, 1880).

(large and small) in most points closely resembling those in which Ross had previously found pigmented cells after feeding on a patient with crescents. So far our results have been negative; but, in view of the peculiar climatic conditions, and of the possibility of the first stage, that of formation of coccidia, being inhibited by the cold, we are not prepared to accept these results as conclusive.

15. With Major Ross, I have examined the organs of some persons (eight) who died of kala azar. This appears to be an infectious disease, indistinguishable at first from malaria. Chronic in character, it continues for months and becomes associated with enlargement of the spleen and liver, and progressive anæmia. The present opinion of most of those who have been deputed to investigate kala azar, as well as of those with longest and most intimate experience of the disease, is strongly in favour of the view that it is malarial in origin.

The melanin or black pigment was absent in the organs of some of the cases I examined; but in all but one yellow pigment was present in the liver, and in most in the kidneys and spleen also, indicating hæmolysis. The iron reaction with acidified potassium ferrocyanide was obtained in the spleen in three instances and, in one, in the liver also.

So abundant and chronic a hæmolysis in cases of malaria, continued moreover after the parasite has ceased to be present (at any rate in sufficient numbers to be found in the peripheral blood or to cause appreciable deposit of melanin in the organs), raises the important question as to the possibility of the differentiation of parasites, with imperceptible morphological differences, by their toxic or hæmolytic properties.

16. Hæmoglobinuric fever seems to have been fairly common of late in some parts of India. I am collecting information, and have requested the editor of the 'Indian Medical Gazette,' to insert in that Journal a series of questions on the subject. Hæmoglobinuria does not occur in kala azar notwithstanding the great amount of hæmolysis which takes place in that disease.

I regret the length of this report, but the main subject of it, Major Ross' researches, cannot be dealt with in a few words, as they supply a basis for our future operations.

[It is necessary to point out that the word "coccidium" has been used by Major Ross and in Dr. Daniel's report above printed in a peculiar and not readily intelligible sense. "Coccidium" is the name of a genus of Sporozoa established by Leuckart in 1879 for the cell-parasite of the rabbit's liver, called *Coccidium oviforme*, and other allied species. "Proteosoma" is the name given by Labbé to another genus of Sporozoa parasitic in the blood-cells of birds. When Major Ross states in his report, dated May 21, 1898, that certain "parasites are

a development in the mosquito of Proteosoma in birds; and to judge from their structure and mode of growth so far as yet observed, I take them to be *coccidia,*" he is using the generic term "coccidium," to describe some phase in the growth of the species of a distinct genus, Proteosoma.

Apparently, what Major Ross intends to indicate by the term "coccidium" is an ovoid firmly walled corpuscle which increases in volume from about 1/2000th inch in length to four or five times that size, and then breaks up into a mass of filiform spores radiating from a central granular mass.

In this mode of spore formation these bodies have resemblances to the true coccidia, which present themselves not only as oviform corpuscles but as cysts with sickle-shaped or filamentous spores. It is, however, not legitimate to apply the generic term "coccidium" to a phase of growth of another genus.—LISTER, *Chairman of the Malaria Committee.*]

OBITUARY NOTICES OF FELLOWS DECEASED.

THOMAS JEFFERY PARKER, eldest son of the late William Kitchen Parker, F.R.S., was born at 124, Tachbrook Street, London, S.W., on October 17, 1850. As a boy he showed a taste for literature and art rather than for science, and this taste was retained throughout life. Being brought up in London, he had little opportunity of developing a taste for outdoor natural history; indeed, in many letters to his friends Parker afterwards referred to this with regret. His subsequent work, and the introduction to the 'Text-Book of Zoology,' written in conjunction with his friend, Professor W. A. Haswell, F.R.S., and completed but a short time before his death, alike prove that his love of Nature was real and that he was fully alive to the importance of a knowledge of the appearance and habits of living things—as a preliminary to the more serious and academic study of the phenomena manifested by them.

Parker received his school training at Clarendon House, Kennington Road, living at home the while, and constitutionally he was never very robust nor much inclined for athletics. On leaving school he entered as a student at the Royal College of Chemistry and Royal School of Mines, where in 1871 he was awarded the Edward Forbes medal and prize of books for biology; and it was the contact with Huxley thus obtained, aided by the influence and loving example of his own father, which moulded Parker's after life. At that time the lectures given in the Royal School of Mines were illustrated only by specimens in the Museum of Practical Geology. Practical work was otherwise *nil* and of Huxley's discourses Parker wrote, "As one listened to him one felt that Comparative Anatomy was indeed worthy of the devotion of a life, and that to solve a morphological problem was as fine a thing as to win a battle." Thus inspired, Parker left Jermyn Street, to fill the office of science master at a school in Yorkshire; but in 1872 he returned to London, on the occasion of the transfer of the Department of Biology of the Royal School of Mines to the building now known as the Royal College of Science, at South Kensington, and shortly afterwards undertook, at Huxley's special request, the Demonstratorship in that subject—an event which marked the turning-point in his career. Writing of the work some years later, he remarked that, "*With the* exception of a fortnight's Science Teachers' Course and

a little instruction from Martin and Bridge, I had to pick up everything as I went on. I feel often quite aghast to think of my utter ignorance of the whole business when I began to demonstrate for Huxley; fully nine-tenths of the things I had never seen until I got into the laboratory, and how the students, to say nothing of the 'General,' stood it ('General' being a term by which Huxley was usually known in the college) is a mystery to me." The working-out of Huxley's splendidly conceived plan of practical teaching of Biology was left largely in Parker's hands, and he was thereby afforded every opportunity for developing his powers both as a teacher and an organiser. Most successfully did he fulfil the task, and in the course of the eight years which he devoted to its development there came under his influence many persons now occupying prominent positions in the biological world, to whom his memory will be ever dear as that of a trusty guide and a true friend. Parker's demonstrations were well worthy the lectures of his great chief; and in the intervals between the courses of instruction he gradually organised a teaching collection, and made, in more direct connection with the work of the laboratory, a number of exquisite dissections with accompanying drawings of representative animal forms. In this it was the good fortune of the present writer to assist, and copies of the drawings, now on the walls of the Biological Laboratory at South Kensington, were in course of time furnished to many of the universities and colleges in the United Kingdom, America, and on the continent of Europe. In this way Parker will ever be remembered as the foremost agent in the development of the Huxleyan method of laboratory instruction; and while it was to him that the more significant modifications undergone by this method up to the time of his leaving England were due, it is interesting to note that of all those who were prominently concerned in its inception, he alone continued to teach both Botany and Zoology to the end of his career.

Early imbued with a desire to emulate his great chief, Parker, in the intervals of official work, commenced writing, and amongst the more popular articles which emanated from his pen may be mentioned the biological portions of the "Recent Science," in the early numbers of the 'Nineteenth Century,' and the article "Carnivora" in 'Cassell's Natural History' (which he wrote in conjunction with his father), together with critical reviews, contributed to the pages of contemporary scientific journals.

Parker's original researches during the period of his service under Huxley were undertaken on his own initiative, the great master being far too engrossed in his own special occupations, and abstracted by polemical and other responsibilities imposed by an eager multitude, to be able to take much personal interest in those working under him. The earlier among Parker's scientific papers, viz., those dealing

with the "Stomach of the Fresh-water Crayfish,"* and the "Stridulating Organ of Palinurus"† bear nevertheless a direct relationship to the work of Huxley's class room. These were followed by others on the "Histology of *Hydra fusca*,"‡ on the "Intestinal Spiral Valve in the genus Raia,'§ and on "Some Applications of Osmic Acid to Microscopic Purposes."‖ In publishing the latter, Parker established for himself a reputation as one of the first to apply the then prevailing zoologists' methods to the preparation of microscopic sections of plant tissues, and he will further be memorably associated with the progress of vegetable histology as having been the first to discover and briefly describe the existence of sieve-tubes in the marine alga *Macrocystis*.¶

While in London Parker became Lecturer on Biology at Bedford College and an Assistant Editor of the 'Journal of the Royal Microscopical Society,' and he served as Examiner in Zoology and Botany at the University of Aberdeen and as an Assistant Examiner in Physiology to the Science and Art Department.

In 1880 he was appointed Professor of Biology in the University of Otago, Dunedin, New Zealand, which office he filled with great credit to himself until the day of his death. Soon after his arrival at the Antipodes, he described** a new Holothurian (*Chirodota dunedinensis*), in promise, as it were, of the magnificent faunistic work since performed by some of those who afterwards became his colleagues in the task of Australian and Novo-Zelandian exploration, and of which we could have wished that Parker had given us more. His mind centred in morphological inquiry, which he continued in full earnest. Among his forty odd published monographs those dealing with the "Anatomy and Development of Apteryx"†† and the "Cranial Osteology, Classification, and Phylogeny of the Dinornithidæ'‡‡ will always be prominent among biological achievements at the Antipodes; but there remain others, such as his later monograph on the "Structure of the Head in Palinurus"§§ and that on the "Myology of the species *P. Edwardsii* (published in the Macleay Memorial Volume, in conjunction with a lady pupil), which link together his work at home and in New Zealand in an interesting association revealing continuity of ideas.

As a teacher, writer, and lecturer Parker was always clear. Unlike

* 'Journ. Anat. and Phys.,' vol. 11.

† 'Zool. Soc. Proc.,' 1878.

‡ 'Roy. Soc. Proc.,' vol. 30, and 'Quart. Journ. Micr. Sci.,' vol. 20.

§ 'Zool. Soc. Trans.,' vol. 11.

‖ 'Roy. Micr. Soc. Journ.,' vol. 2.

¶ 'N. Z. Inst. Trans.,' vol. 14.

** 'N. Z. Inst. Trans.,' vol. 13.

†† 'Phil. Trans.,' 1892.

‡‡ 'Zool. Soc. Trans.,' vol. 13.

§§ 'N. Z. Inst. Trans.,' vol. 16.

his father, who "was a seer but not an expositor," he was logical in his methods, and was careful to present his facts and arguments in fitting sequence.

In literary style Parker seemed to have been largely influenced by Huxley, Matthew Arnold, and Russell Lowell, many of the writings of each of whom he knew almost by heart, and the reports of some of his popular addresses do credit to his choice. This may be truly said of a speech made by him on the occasion of the distribution of prizes at the Otago Boys' High School, on December 13, 1894, which was enlivened by a witty vein of rare merit, and showed Parker to have been possessed of a keen sense of humour. Soon after his arrival in Otago he delivered his "Inaugural Address," taking as his subject 'Biology as an Academic Study.' In this he ventured to insist on the importance of Darwin's work. But at that time the doctrine of evolution was apparently looked upon by the inhabitants of Dunedin as a bad form of heresy, and the address, as well as a lecture he gave some two years later, on Darwin, produced a storm in the local newspapers. Parker, however, prevailed, and his subsequent addresses and lectures on biological and educational subjects show him to have been intent on progressive measures. Pursuing these with a literary facility, quiet humour, and with common-sense views on general educational questions, we find him extending his influence beyond the limitations of his own department in his University, and becoming largely responsible for the introduction of many improvements in the Degree Regulations which have been to the advantage of all concerned. He was a strong advocate of higher educational aims, and lost no opportunity of insisting on the importance of post-graduate study. With this in view, he instigated several of his students to undertake research, and established in connection with their work a series of 'Studies in Biology for New Zealand Students,' the chief among which appeared as contributions to the publications of the Museum and Geological Survey Department of that Colony.

The duties of Professor of Biology at Dunedin include the Curatorship of the large and important museum of the University, and in this work Parker showed an exceptional talent. In addition to arranging the collections already there, he from time to time added to them, and greatly developed them along modern and improved lines. Before leaving England he had been led to experiment on methods of preservation, it being a desire of his artistic nature to ensure if possible the retention of their natural colours by museum-preserved animals. In this he did not succeed, but in seeking to conserve cartilaginous skeletons so that they might be examined high and dry, he achieved a notable result by the employment of a glycerine jelly method. Conspicuous among his labours in this direction skeletons of *Carcharodon*, *Callorhynchus*, *Notidanus*, and Torpedo, together with numerous lesser

preparations, may be seen in the British Museum of Natural History, the Cambridge University Museum, and elsewhere at home and at the Antipodes, which have stood the test of prolonged display and exposure to the action of light and air. Mainly in association with his curatorial work, there appeared from time to time in the pages of 'Nature' a series of "Notes from the Otago Museum," which, while serving as a record of his manipulative experience, embody important observations and discoveries, and have proved of great service to *préparateurs*.

As a writer of books, Parker was no less successful than in his other vocations, and in a charming biographical sketch of his father, written in 1893, he realised a high literary standard. His first published volume, a 'Course of Instruction in Zootomy (Vertebrata),' appeared in 1884, three years after his settlement in New Zealand. It was, however, for the greater part prepared before he left England, and with the exception of Huxley and Martin's 'Elementary Biology,' in the final revision of the proofs of which Parker had a hand, it was the first in the field among laboratory treatises of the now familiar didactic order. During the preparation of the earlier part of this highly successful work, Parker materially assisted Huxley with the anatomical portion of the latter's ideal book upon 'The Crayfish,' at that time in course of preparation. It is doubtful if there has ever appeared a more perfect treatise upon any one single organic being than this; and Parker, through it and his earlier published papers conspicuously associated with Huxley's epoch-marking labours in scientific education, seemed under the hand of fate concerning the Crayfish and its allies; for in his 'Skeleton of the New Zealand Crayfishes,' which appeared in 1889, he developed most conspicuously his own ideas of laboratory organisation. Parker's powers of exposition of his subject may best be judged by his 'Lessons in Elementary Biology' (1891). The scheme for this book was already in his mind while demonstrating under Huxley, and the work itself, at present in its third edition, has been translated into German, and now occupies the foremost position among all elementary treatises of biology not intended for laboratory use.

Parker's affectionate nature and charm of personal manner endeared him to wide circle of friends, and amongst his students he was a general favourite. His unassuming character, and his literary, artistic, and musical tastes, resulted in a wide sympathy with all sorts and conditions of men. He took an active part in the social life of Dunedin and was President of the Savage Club, as well as of the Otago branch of the New Zealand Institute. He was elected a Fellow of the Royal Society in 1888, and in 1892 was granted the degree of D.Sc. *in absentia* by the University of London. He was a Corresponding Member of the Zoological Society of London and of the Linnean Society of New South Wales, an Associate of the Linnean Society *of London*, of which he became a Fellow at about the time of his

death, and he was a Member of the Imperial Society of Naturalists of Moscow. The gradual decline of his wife's health did much to sadden many years of Parker's life, and some little time after her death, as well as that of his father and mother, symptoms of an organic ailment became apparent, to which he eventually succumbed. But he had learned to bear his burden quietly and with manliness, and in spite of trouble sufficient to crush many a stronger person, he showed a cheerful face to the world, and found happiness in his work and home-life. In the autumn of 1892 he came on a visit to England, intending to visit the European Museums for the examination of remains of the Dinornithidæ, in order to complete his monograph on that group. At the end of the time he had the great delight of spending a few days at Eastbourne with his old chief, of his admiration for whom he afterwards wrote :—" Whether a professor is usually a hero to his demonstrator I cannot say; I only know that, looking back across an interval of many years and a distance of half the circumference of the globe, I have never ceased to be impressed with the manliness and sincerity of his character, his complete honesty of purpose, his high moral standard, his scorn of everything mean or shifty, his firm determination to speak what he held to be truth at whatever cost of popularity. And for these things I loved the man, and do honour to his memory, on this side idolatry, as much as any."

Parker's last completed piece of work was his aforementioned 'Text-Book of Zoology,' written in conjunction with Professor W. A. Haswell, F.R.S., of the Sydney University. This was begun in 1892, and though all the proofs were corrected before his death, he did not live to see it published. The original plan of this beautifully illustrated book, the clearness of the well-balanced descriptions, as well as of the parts dealing with the wider and more general aspects of the subject, place it in the front rank of elementary zoological text-books; and throughout the work it is evident that the authors have been more careful to supply what is best for the beginner than to impress the reader with their own wide acquaintance with details. A shorter form of this book was in course of preparation at the time of Parker's death, and he had nearly completed half the manuscript for a 'Biology for Beginners,' and was making plans with his brother, Professor W. N. Parker, for the preparation of an 'Elementary Practical Zoology.' He had also begun, in conjunction with Mr. J. P. Hill, of Sydney University, an investigation on some Emeu chicks, and had obtained interesting results.

In the autumn of 1895, Parker suffered from a bad attack of influenza, and the following year he paid a visit to Sydney; but the journey was apparently beyond his strength. A second attack of influenza in the summer of 1897 completely prostrated him, and was followed by serious symptoms. Again and again he tried to resume

work, but each attempt resulted in a relapse, and at the close of the academic session, towards the end of October, it was thought best for him to try the effects of a complete change. Accompanied by his sister, who had joined him and his three boys in Dunedin shortly after the death of his wife, he started on a visit to his friend, Mr. Bell, of Shag Valley, about forty miles from Dunedin. But halfway there he was so prostrated that the continuation of the journey had to be postponed, and a week later it was decided to return to Dunedin by easy stages. After a night's rest at Warrington, he seemed to be better, but the same night he began gradually to sink, and died a few days later—on November 7, 1897. He was buried at Warrington, and a number of his Dunedin friends accompanied him to the grave. His unexpected death, at the age of 47, is a severe loss to biological science in the Antipodes, where he was one of its foremost pioneers.

G. B. H.

PERCIVAL FROST was born at Kingston-upon-Hull on September 1, 1817. He was the second son of Mr. Charles Frost, F.S.A., who practised as a solicitor in that town. Percival Frost's earlier school-life was spent at Beverley. From Beverley he was removed in the year 1833 to Oakham School, which was then presided over by Dr. Doncaster, and here he remained until October, 1835, when he proceeded to St. John's College, Cambridge.

As an undergraduate, Frost devoted most of his energies to the study of mathematics. The competition which Frost met with at St. John's College is sufficiently apparent from the fact that in his year, 1839, the first four places in the Mathematical Tripos were won by men of his own college; a unique example of one college obtaining the first four places in that Tripos. Frost's chief rival was B. M. Cowie, the present Dean of Exeter. In the Mathematical Tripos, in January, 1839, Cowie was Senior Wrangler, and Frost was Second Wrangler; but immediately afterwards this order was reversed by the examiners for the Smith's Prizes. Both were elected to fellowships in their College on the same day, 18th March, 1839.

After his degree, Frost was urged by friends, and especially by Dr. Hymers, his college tutor, to read for the Bar, and he commenced to do so; but his great success in obtaining private pupils when he returned to Cambridge for the Long Vacation, induced him to abandon all idea of the legal profession. In 1841 Frost was ordained by the Bishop of Ely, and in the same year vacated his fellowship on his marriage with Jennett Louise, daughter of Mr. Dixon, of Oak Lodge, Finchley, the commencement of a happy union which lasted 57 years. Frost held a mathematical lectureship in Jesus College from 1847 to 1859, and one in King's College from 1859 to 1889; but his chief work consisted in the tuition of private pupils.

In this work he was eminently successful; many of his pupils took high degrees. As examples of those who rose to distinction at the Bar and in Science, may be mentioned the names of Lord Justice Rigby and the late W. K. Clifford.

The first book which Frost wrote was an edition of Newton's 'Principia,' Book I, sections 1—3 (with notes and illustrations, and a collection of Problems); it was published in 1854. Subsequent editions appeared in 1863, 1878, and 1883. His next work he published in 1863, in conjunction with the late Joseph Wolstenholme. It was entitled 'A Treatise on Solid Geometry.' Second and third editions of this work were published by Frost alone in 1875 and 1886, and 'Hints for Solution of Problems in the Third Edition of Solid Geometry,' in 1887. In 1872 he published his third work, 'A Treatise on Curve-tracing.' In addition to these books, he wrote a considerable number of minor papers relating to Algebra, Analytical Geometry, the Lunar and Planetary Theories, and Electricity and Magnetism, more than twenty of which appear in this Society's 'Catalogue of Scientific Papers.'

In 1882 Frost was elected a Fellow of the Royal Society, and in the same year he was elected by King's College, Cambridge, to a terminable Fellowship, to which he was re-elected three times, and which he held at the time of his death. By new University Statutes, which came into force in 1882, two new degrees were established at Cambridge, those of Doctor of Science and Doctor of Letters. Shortly afterwards, Frost proceeded to the degree of Sc.D.

Frost was no mere mathematician; he was a man of wide interests and varied attainments. He had an extensive acquaintance with the works of musical composers, and his execution on the pianoforte was of a high order. His drawings in water colours were very successful. Moreover, he had a Yorkshireman's instinctive love for games and sports.

Frost possessed a strong constitution, and enjoyed excellent health. If we make an exception of the lameness of his later years, against which he courageously fought, he scarcely knew, until he had passed his 80th birthday, what a day's illness was. Towards the end of last April, he was attacked by a painful disorder, which in six weeks' time proved fatal to a frame exhausted by prolonged suffering. Frost died on Trinity Sunday, June 5, 1898, and his remains were laid to rest on the following Friday in the Mill Road Cemetery, Cambridge.

Frost's character endeared him to all who knew him. He was admired and esteemed by his pupils; and between him and them many a life-long friendship was established. His kindness of heart and consideration for others could not be exceeded. If there was anything that he abhorred, it was what seemed to be self-conceit and pretentiousness. He was always bright and cheerful, and ready to see fun in any situation which might occur.

The writer of these lines takes this opportunity of recording his own deep debt of gratitude to Dr. Frost. Some four years ago, when the writer ceased to be able to read, Dr. Frost with characteristic kindness and generosity volunteered to act as reader. His readings, which took place three or four times a week, and lasted about an hour and a half each, were continued until Dr. Frost was attacked by his fatal illness. In that period something like thirty octavo volumes, on subjects of diverse interest, were read, as well as a sprinkling of special articles from the 'Times,' or papers in 'Nature' and other scientific periodicals.

The memory of such a friend cannot easily fade.

H. M. T.

Lyon Playfair, son of Dr. George Playfair, Chief Inspector General of Hospitals, Bengal, was born at Meerut, May 21, 1819. He came home for his education to St. Andrews, where his grandfather had been Principal of the United College of St. Salvator and St. Leonard, and where his uncle, Sir Hugh Lyon Playfair, after a distinguished career in the Indian army, retired in 1834, not to repose but to new battles against dirt, disorder, and ruin, battles the result of which we see in the clean, prosperous, and healthy city of St. Andrews. We may well believe that Lord Playfair derived some of his enthusiasm for sanitation and order from this uncle, "the eccentric and energetic soldier who begged and bullied and wheedled away the filth and ruinous neglect which bade fair to entomb the ancient city." After some years in St. Andrews, he went to Glasgow to study medicine, but was attracted to chemistry by the teaching of Thomas Graham, then Professor of Chemistry in the Andersonian. After a short visit to India he resumed his chemical studies, under Graham, in the University College, London. In 1838 he went to Liebig's laboratory at Giessen, where he worked at organic chemistry and produced his first scientific paper "On a new Fat Acid in the Butter of Nutmegs." Liebig was not only his teacher but his friend, and when Liebig, on the invitation of Prince Albert, came to this country to lecture on agricultural chemistry, Playfair acted as his assistant and interpreter, and was thus introduced to the Prince, an introduction which had an important effect on his subsequent life.

For two years he managed the chemical department of Messrs. Thomson's print-works, at Clitheroe. In 1843 he was appointed Professor of Chemistry in the Royal Institution, Manchester. In 1844, on the recommendation of Sir Robert Peel, he was appointed a member of a Royal Commission for the examination of the sanitary condition of large towns and populous districts. This was the beginning of what was to be a large part of the work of his life. In 1845 he was one of *the commissioners* on the Irish famine, and from that time till his death

there was no year during which he was not appointed to serve on a Royal Commission or a Select Committee of the House of Commons, in very many cases as chairman. Among the Commissions on which he served, besides those already named, may be mentioned—Exhibition of 1851, Exhibition of 1862, the Cattle Plague, the Reorganisation of the Civil Service (the report of which is still known officially as "The Playfair Scheme"), Pensions for the Aged Poor, the University of London, the Herring Fisheries of the United Kingdom, Coal for the Navy.

In 1846 he was appointed chemist to the Museum of Practical Geology and Professor of Chemistry in the Government School of Mines.

As Special Commissioner in charge of the Department of Juries at the great Exhibition of 1851, Playfair had an entirely new task before him. This was the first International Exhibition, he had no precedent to work upon, what he did was quite original, and it was so well done that it became the model for all succeeding international exhibitions. There can be no doubt that the success of the 1851 Exhibition was to a great extent due to Playfair's clear view of what ought to be done and of what could be done, and to his untiring energy in doing it and in getting other people to do it. The value of this work was recognised in the highest quarters and Playfair became a Companion of the Bath, and an officer in the household of the Prince Consort. A more striking proof of the value set by others on his services was the fact that he was asked to undertake the same duty in connection with the Exhibibition of 1862, as also that at the Paris Exhibition in 1878, the Prince of Wales, who was President of the British Commission, appointed him chairman of the Finance Committee.

The 1851 Exhibition led in 1853 to the foundation of the Department of Science and Art, and Playfair and the late Sir Henry Cole were appointed joint secretaries. In 1856 Playfair became Inspector-General of Government Museums and Schools of Science. These offices he held till 1858, when on the death of Professor Gregory he was appointed to the chair of Chemistry in the University of Edinburgh. In Edinburgh he created, practically out of nothing, a really useful teaching laboratory. The rooms then available were very ill-suited for the purpose and the funds quite inadequate, but he made the most of the former and supplemented the latter, spending on the department the whole of his professorial income during the first year and a large part of it for several subsequent years of his tenure of office. The University of Edinburgh is also indebted to Playfair for the introduction of degrees in science. In 1868 Playfair was returned as the first representative in Parliament of the Universities of St. Andrews and Edinburgh. He was Postmaster-General in 1873, and Chairman of Ways and Means and Deputy-Speaker from 1880 to 1883. On his retirement from this office he was made K.C.B. At the general

election in 1885 he was returned for the Southern Division of Leeds and was appointed Vice-President of the Council on Education. He continued to represent Leeds until he was raised to the Peerage in 1892.

Playfair was an original member of the Chemical Society of London, over which he presided in 1857—1859. He was elected a Fellow of this Society in 1848, and of the Royal Society of Edinburgh in 1859. He was President of the Chemical Section of the British Association in 1855 and in 1859, and of the Association in 1885. He was an honorary member of many foreign learned bodies and held many foreign decorations. He died in London, on Sunday, May 29, 1898.

Playfair had a truly scientific mind and was always busy, and yet we do not find a great deal of original scientific work recorded under his name in the 'Royal Society Catalogue of Scientific Papers.' His work lay mostly in another direction. As he belonged not only to the world of science but also to that of practical business, he was specially fitted to act as an interpreter between them. Such an interpreter is needed. The man of science does not always know what the business man wants, and the business man often does not understand what the man of science tells him. Such services are perhaps appreciated more highly by the man who immediately feels the benefit of them, the statesman, the manufacturer or the merchant, than by the man of science, but we should remember that if science takes a higher place now than it took fifty years ago, if the opportunities for the genuine study of science and for the prosecution of scientific investigation are greater now than they were then, if science is taking more nearly its right place in the education of the country, that is due to a large extent to Playfair's wisdom and hard work. Of Playfair's contributions to pure chemistry the most important is the discovery and investigation of the nitroprussides, and to applied chemistry, the report on the work undertaken by him along with Bunsen on the gases evolved in iron furnaces. But besides what was published in scientific journals, or in the Transactions of learned societies, Playfair did a great deal of original scientific work, how much no one can now tell, incidentally in the course of the investigations of the numerous commissions of which he was a member.

A. C. B.

Brigade-Surgeon JAMES EDWARD TIERNEY AITCHISON, M.D., C.I.E., F.R.S., F.L.S., LL.D.,* died at Kew, on September 30, 1898, after a considerable period of suffering from a weak heart and other diseases. He was a son of the late Major James Aitchison, and was born at

* Much of this notice is word for word the same as one I drew up for 'Nature' and the 'Kew Bulletin.'—W. B. H.

Neemuch, Central India, on October 28, 1835. His education was begun at the parish school of Lasswade, Midlothian; thence he went to the Grammar School at Dalkeith, and subsequently to the Academy and University of Edinburgh. After graduating M.D. and L.R.C.P. in 1856, he entered the service of the Honourable East India Company, as Assistant-Surgeon, in 1858, and retired in 1888. We have no particulars of his early life, but he seems to have taken up botany soon after his arrival in India, for in 1863 he published an account of the 'Flora of the Jhelum District of the Punjab.' This was followed by a 'Catalogue of the Plants of the Punjab and Sindh,' in 1869, and other papers on economic and geographical botany. He had already long been in communication with Kew, where his first collection of dried plants, comprising between 300 and 400 species, was received in 1862. These plants were from the districts named in the foregoing titles, and included little that was actually new to science; but the specimens were so carefully selected and so well dried that they were valuable on that account. In 1872 he was appointed British Commissioner to Ladak, where he continued collecting on a small scale, and transmitted his plants to Kew.

Dr. Aitchison's more active career in scientific pursuits began, however, when he accompanied the troops under General (now Lord) Roberts into the Kuram Valley, Afghanistan, in 1878, when he served with the 29th Punjab Regiment, Native Infantry. The following year he was attached to the force as botanist, and during 1879 and 1880 he very thoroughly explored the country from Thal to the Shutar Gardan, at elevations ranging from 2,000 feet up to 13,000 feet, on Mount Seratigah, and 15,000 feet on Mount Sikaram. The collection of dried plants of 1879 consisted of 950 species, represented by 10,000 specimens, and was published in the eighteenth volume of the 'Journal of the Linnean Society.' Nearly as large a collection was made in 1880, and this was published in the nineteenth volume of the same Journal. Subsequently, Dr. Aitchison was appointed Naturalist to the Afghan Delimitation Commission, and on that expedition, during 1884–85, he made his most important collections, both botanical and zoological.

The route was from Quetta through Northern Baluchistan, and thence northward, touching the Helmund, in about 63° longitude; up this river, onward into the valleys of the Harut and Hari Rud rivers, and thence to Meshed. Subsequently an excursion was made into Russian Turkestan, as far east as the Morgab river.

The country traversed is noted for its vegetable productions, especially drugs, many of uncertain origin, and although he made a general collection, Aitchison applied himself, successfully, to the investigation of their sources. His botanical collection on this journey comprised about 800 species, and 10,000 specimens. It is the subject of a memoir *in the* 'Transactions of the Linnean Society' (2nd series, Botany,

vol. 3), illustrated by fifty plates. The gum-yielding Umbelliferæ, of which he brought home a magnificent series of specimens, form a special feature of this memoir.

The zoological collection, though less comprehensive, included a considerable number of novelties; and was also published in the 'Transactions of the Linnean Society' (2nd series, Zoology, vol. 5), and illustrated by a number of plates. Each of the papers to which reference has been given, is preceded by an essay on the vegetation and vegetable products, both wild and cultivated, of the countries explored, and thus contains much valuable information. It ought to be added that these collections were made under very great difficulties, such as would have discomfited a man of less determination and endurance. He loved his beautiful specimens, and handled them as though they were the most delicate organisms. They are now incorporated in the herbaria of no fewer than sixteen different establishments.

Aitchison was of an enthusiastic and energetic temperament, and of an amiable and warm-hearted disposition, and many will feel the loss of so true a friend. Much of his success in collecting in a hostile country was due to his kindness to the natives, especially the sick, whom he treated medically or surgically. Such was his reputation, that it preceded him and ensured him a friendly reception.

For the Kuram campaign Dr. Aitchison received the medal and clasp; in 1882 he was elected a Fellow of the Royal Society of Edinburgh; in 1883 he was elected a Fellow of the Royal Society of London; and in the same year he was created a Companion of the Order of the Indian Empire. In 1892 he unsuccessfully contested the seat in Parliament for Clackmannan and Kinross in the Liberal Unionist interest. During the last years of his life he was engaged collecting materials for a 'Flora Indiæ Desertæ,' that is of North West India, Afghanistan, and Baluchistan; but his sufferings prevented him from working them out.

W. B. H.

Mr. OSBERT SALVIN, was born at Elmshurst, Finchley, on the 25th of February, 1835, being the second son of the late Mr. Anthony Salvin, the well-known architect, and Anne, daughter of the Rev. Wm. Nesfield, Rector of Brancepeth, in the county of Durham. Early in the year 1852, the front of Trinity Hall, in Cambridge, was destroyed by fire, and the professional services of Mr. Salvin were employed in rebuilding that part of the College. The result of this connexion between him and its authorities was, that in 1853, he placed his son, who (after a preparatory course at the Manor House, Finchley, kept by the Rev. Charles Worsley) had just left Westminster School, under their care, and the choice was justified by the latter obtaining a scholarship at the end of his first year. While at college, however, he was considered

not to do justice to his abilities; and, though he graduated as a Senior Optime in the Mathematical Tripos of 1857, it was thought that he could easily have secured for himself a much higher place. The truth is, that being a naturalist born—as a child his delight was in gathering wild plants, and bringing them to his elders to be named—he devoted far more time and attention to Natural History than to Mathematics, and diligently worked, so far as opportunity would allow, at Zoology and Geology—birds and insects being his favourite study in the former, and in the latter science the palæontological branch. Rowing was also another recreation, and he pulled the seventh oar in the boat sent by his college to Henley in 1856. Being singularly apt with his fingers, he found much occupation in carpentry and machinery—indeed, while at Westminster, he and his elder brother (his senior by a few years only) built and fitted two small steamers, which worked so efficiently that they were bought to be used on some of the rivers in India. With all these distracting tastes, it is not surprising that Osbert Salvin should have studied mathematics only enough to ensure his attaining a respectable degree,* and eventually the practical pursuit of zoology asserted itself almost to the exclusion of its rivals. Coming from Westminster he naturally had many friends among his old schoolfellows, who had joined the Third Trinity Boat Club, composed wholly of men from that school and Eton, and thus he came to know Mr. Frederick Godman (an Etonian), with whom he was subsequently to become so intimate a fellow-worker; while he also formed a close acquaintance with Mr. (afterwards Sir) Edward Newton, of Magdalene College, an enthusiastic ornithologist, through whose means Mr. Salvin was introduced to Mr. W. H. Hudleston (then Simpson). With this gentleman Mr. Salvin, immediately after taking his degree, set out to join Mr. (now Canon) Tristram, who was by marriage his second cousin, in the Natural History Exploration of Tunis and Eastern Algeria, where the party passed five months, throwing an abundance of light on the zoology of those countries, as the accounts published in 'The Ibis' for 1859 and 1860 shew. Soon after his return from this expedition, which will always be memorable in the annals of Ornithology, Mr. Salvin prepared to go to Central America, and in the autumn of 1857, proceeded to Guatemala, in company with the late Mr. George Ure Skinner, the celebrated discoverer and importer of Orchids, staying in that country till the middle of the following year, when on his way home he for a short time joined Mr. Edward Newton, then in the Antilles. A few months later, Mr. Salvin returned to Central America, henceforth always to be associated with his name, since there he proved himself to be unsurpassed as a collector, though those were the days of Bates and Wallace. Like those great naturalists, he used intelligence in his col-

* *A place in the Natural Sciences Tripos of his day did not of itself admit to a degree, or he would probably have graduated in that way.*

lecting, and the several papers that he published (far too few), telling of his experience in that country, bear abundant evidence of the reflective and trained mind of the observer, as well as his moral perseverance and physical endurance, in proof of which may be especially cited his articles in 'The Ibis,' on collecting *Trochilidæ* at Dueñas and other places, and on the habits of the Quezal or Resplendent Trogon (*Pharomaceous mocinno*) in Vera Paz. Returning to England in May, 1860, he again went out in the autumn of 1861, this time accompanied by Mr. Godman, continuing with greater success than before his former explorations, and ere the year was out had twice ascended the southern or fire peak of the Volcan de Fuego, near the city of Guatemala ('Athenæum,' No. 1793, March 8, 1862, p. 331). The collections made in this tour, which did not end till January, 1863, were very large, and comprised every class of the Fauna, while the Flora was not neglected, and many of the ruined temples and other remains of antiquity were visited and photographed. Soon after his return home, Mr. Salvin was induced to undertake the management of some engineering works in the north of England, but this employment, which he found very distasteful to him, did not last long. In 1865 he married Caroline, the daughter of Mr. W. W. Maitland, of Loughton, in Essex, and sister of an old friend and contemporary at Trinity Hall, Mr. John Whitaker Maitland, and, in 1873, set out with her on another voyage to Central America, returning by way of the United States, chiefly with the object of examining the collections in the Museums of Washington, Philadelphia, New York, and Boston. In 1874, on the foundation of the Strickland Curatorship of Ornithology in the University of Cambridge, he accepted that office, which he filled until 1882, when, his father having died in 1881, he succeeded to the small but beautiful property at Hawksfold, near Haslemere, whither he removed, making it his permanent residence, though there was scarcely a week some days of which he did not pass in London, for he and Mr. Godman had conceived the idea of bringing out a 'Biologia Centrali-Americana,' being a complete Natural History of all the countries lying between Mexico and the Isthmus of Panama. This gigantic task—by far the greatest work of the kind ever attempted—taxed all their united efforts as well as those of the many contributors they enlisted. The botanical part has been completed, but the zoological portion, and that by Mr. Maudslay on the antiquities, are still in progress. Before beginning this, Mr. Salvin had edited the third series of 'The Ibis,' of which he had been, in 1858, one of the founders, and had brought out a 'Catalogue of the Strickland Collection of Birds in the Cambridge Museum,' which was published at the University Press. His earliest contributions to scientific literature, while still an Undergraduate, were to 'The Zoologist' for 1856 (p. 5278) and 1857 (p. 5593), and shew the precise regard for accuracy which throughout his

life was one of his chief characteristics. He also joined Mr. Sclater, who had long been working on the birds of South and Central America, in the publication of 'Exotic Ornithology' (a series of plates and accompanying memoirs—limited however to forms of the New World) and of the 'Nomenclator Avium Neotropicalium' (1873). He further contributed the *Trochilidæ* and *Procellariidæ*—on which last group he became the acknowledged authority—to the British Museum 'Catalogue of Birds' (vols. xvi and xxv), and almost his latest labour was that of completing and arranging the late Lord Lilford's 'Coloured Figures of British Birds,' while this Society's 'Catalogue of Scientific Papers,' enumerates forty-seven published by Mr. Salvin alone, fifty-four by him and Mr. Sclater jointly, and twenty-three by him and Mr. Godman. His chief entomological work, for the most part executed in conjunction with the gentleman last named, and ostensibly limited to the *Rhopalocera*, is to be found in their grand undertaking; but there are probably few pages in that publication which do not bear silent witness to his careful supervision.

Mention has been made of his skill in carpentry, and this was not without a very useful result. For several years he, with his own hands, constructed the cabinets needed to hold his ever-increasing collections, and was thereby led to think out a scheme for overcoming what almost all collectors had hitherto found to be a serious hindrance—the inconvenience produced by having, when arranging a collection systematically, to interpose shallow among deep drawers, or the converse, owing to the different size of the specimens to be housed, and causing in many cases a great waste of space. He devised a system of cabinets in which the drawers, each being a multiple of the same unit, became practically interchangeable. His plan, simple enough in theory, involved several ingenious improvements and adaptations, such as a technical expert only could supply, before it was perfected. Having been adopted by some of his private friends, it was introduced into the Museum of Zoology at Cambridge, and afterwards, with a modification, whereby the chief advantage of the original idea was lost, into the Natural History portion of the British Museum. Its use has since been very generally copied; for its merits, when understood, are obvious.

Elected to this Society in 1873, Mr. Salvin was also a member of the Linnean, the Zoological and the Entomological Societies, on the Councils of all of which he frequently served; and it may be truly said that there were few naturalists whose opinion was more often sought, for his advice was generally sound. His figure was well known at the Athenæum Club, and last year he was elected an Honorary Fellow of his old College. He had suffered for several years from an affection of the heart, and was well aware of the precarious tenure of his life. He continued in his usual condition of health until a few

days before his death, which took place at his house at Hawksfold, on the 1st of June, 1898. He is greatly missed by a large circle of friends, to whom his kindly nature and unassuming manner, to say nothing of the breadth of his scientific views, had greatly endeared him.

A. N.

JOHN HOPKINSON was born on July 27, 1849, son of Alderman Hopkinson, of Manchester, and of a daughter of Mr. John Dewhurst, of Skipton. He was the eldest of a distinguished family of brothers. Alfred Hopkinson, Q.C., is now Principal of Owens College, Charles Hopkinson and Edward Hopkinson, are engineers, and Albert Hopkinson is a doctor of medicine.

After an early training at Lindow Grove School and Queenwood, John entered Owens College before he was 16 years of age. He showed marked taste and capacity for mathematics and physics, and at the age of 18 went on to Cambridge, entering Trinity College. His academic career was one of particular distinction. In 1871 he was Senior Wrangler and first Smith's Prizeman, having meantime taken the London degree of D.Sc., as well as a Whitworth Scholarship. He was elected to a fellowship at Trinity, but he went down from the University immediately after taking his degree to become an engineering pupil in the works of Messrs. Wren and Hopkinson, where his father was a partner. A very short time spent there sufficed to complete his preparation for professional work, for in 1872 he entered the service of Messrs. Chance Brothers and Company, glassmakers, of Birmingham, as their engineering manager. An important section of Messrs. Chance's work related to lighthouse illumination, and in this field Hopkinson's scientific genius at once found scope. He set himself to the improvement of dioptric lights and introduced the system of producing a group of flashes by the rotation of the apparatus, for the purpose of giving a wider variety to aid sailors in distinguishing between different lights. In 1874 he issued a pamphlet pointing out the advantages of group-flashing lights and showing a simple dioptric apparatus suited to produce them. He also pointed out how easily the group flash could be given with catoptric apparatus. The system has found extensive application. It was applied for the first time in 1875 to the catoptric floating light on the Royal Sovereign Shoals, near Beachy Head, and has since been applied to several lightships by the Trinity Corporation. The first land light on Hopkinson's system was made in 1875, for Tampico Lighthouse, in the Gulf of Mexico, and this was soon followed by many more. Later, when the question arose of adopting electricity in lighthouses, Hopkinson's work did much to overcome the difficulties which attended the use of the new illuminant, and several of the early electric lighthouses were equipped *to his designs.* Probably the designs of no lighthouse engineer have

been more varied, or more uniformly successful than his. Two of his more considerable works—the lights of Macquarie and Tino—are described in a paper which he read before the Institution of Civil Engineers, in 1886.

In 1878 he removed from Birmingham to London to practice as a consulting engineer. But his connection with Messrs. Chance was not broken, and he remained for many years their scientific adviser. Lighthouse design, however, was only one of several fields of work in which the influence of his originality was coming to be strongly felt.

At the time of Hopkinson's removal to London the dynamo electric machine had just ceased to be regarded as little more than a scientific curiosity. Its application to electric lighting had begun; the possibility of reversing its function and using it as a motor had been pointed out; but the conditions which should govern its design were very imperfectly understood, and the patterns of machine then manufactured were crude, clumsy, and wasteful. It is to Hopkinson more than to any other man that the modern dynamo owes its efficiency. His first published work on the subject is to be found in two papers on electric lighting, which were read and discussed before the Institution of Mechanical Engineers in 1879 and 1880. These papers describe experiments made by the author on a Siemens dynamo, to determine the relation of the electrical output to the power expended in driving the machine. The relation of current to potential was exhibited graphically in a form which has since been widely used by electrical engineers under the name of the "Characteristic curve." This pioneer work was followed by a series of magnetic researches which paved the way for a general theory of the magnetic circuit of the dynamo machine published by the brothers John and Edward Hopkinson in the 'Philosophical Transactions' for 1886. The principles then laid down, were of fundamental importance, and their influence on design was revolutionary. Hopkinson himself was the first to apply them in practice. Taking as the basis of his operations the form of machine designed by Edison, he modified it in accordance with the theory he had demonstrated, and the Edison-Hopkinson dynamo, with its improved armature and greatly shortened magnetic circuit, was speedily accepted not only as a machine of extraordinary merit, but as the embodiment of principles guiding all dynamo design.

Hopkinson was now in the full swing of his work as an electrical engineer, inventor, and expert. Among other inventions which appear in his numerous patents are the closed circuit transformer, the three wire system of electric distribution, and the series-parallel system of motor working in electrical railways and tramways. His inventions bear striking evidence of his scientific prescience; in several instances they were made too soon to bring him much or any return. His professional success however was great. At an unusually

early age he attained an almost unique position as an engineering consultant, mainly but by no means wholly in electrical matters. His frequent appearances in the law courts as expert witness represented only one side of a busy and varied professional life. In recent years his engineering work has concerned itself much with electric traction as well as with the carrying out of large schemes of electric lighting. He was engineer of the Manchester electric supply, and of electrical tramways at Leeds and Liverpool. He took part, as a member of Council, in the management of the three great engineering societies —the Institutions of Civil, Mechanical, and Electrical Engineers. The Electrical Engineers twice made him their President (in 1890 and 1896). His presidential address in 1890 was devoted to a review of results of magnetic research, remarkable, as indeed all his papers were, for its lucid brevity and comprehensiveness. In his address in 1896 he proposed the formation of a volunteer corps of electrical engineers. The corps was formed and he was himself its first Commanding Officer.

Throughout this active professional life, Hopkinson made time for a remarkable amount of purely scientific work, much of which was of first-rate importance. His published papers began to appear as early as 1871. They number in all about sixty-five. Ten of them are to be found in the 'Philosophical Transactions,' and many more in the 'Proceedings' of the Society.* About half of the whole number deal with magnetism, and with the applications of electricity and magnetism in engineering. It was in this field that Hopkinson accomplished the part of his work that is most widely known. Before these subjects engaged his attention, however, he had broken other ground which he continued to cultivate at intervals for many years. His earliest papers refer to miscellaneous problems in elasticity—to the rupture of an iron wire by a blow, and to the stresses produced in a disc by rapid rotation. His connection with Chance's works led him to investigate the refractive indices of glass, and this led on—through the connection afforded by Maxwell's theory of light—to a prolonged research dealing with electrostatic capacity and the phenomena of residual charge. Two papers on the residual charge of a Leyden jar were published in the 'Transactions' in 1876 and 1877 and were followed by others on the electrostatic capacity of glass and liquids,† and on specific inductive capacity,‡ and by a final paper "On the Capacity and Residual Charge of Dielectrics as affected by Temperature and Time."§ It is impossible, in a few sentences, to give any adequate summary of this important

* It is satisfactory to know that a collected edition of Hopkinson's papers will be published by the Cambridge University Press.

† 'Transactions,' 1877 and 1880.

‡ 'Proceedings,' 1886 and 1887.

§ 'Transactions,' 1897.

section of Hopkinson's original work. It was shown that a dielectric which had been subjected to successive electromotive forces of opposite polarity gave a corresponding inverted succession of residual discharges, and these were treated in the manner of Boltzmann's theory of the after-effects of mechanical strain. The specific inductive capacity of many substances was measured in the earlier experiments for periods ranging from 1/2 to 1/20,000 of a second, and was compared, on Maxwell's theory, with the refractive index for long waves. It was shown that in hydrocarbon oils the results were in agreement with Maxwell's theory, but with other substances there was generally disagreement. The last paper is of particular interest as supplying a key to this discrepancy. It describes experiments on ice, and on glass at various temperatures under high as well as low frequencies of charge and discharge, the high frequency ranging from 2,500,000 down to 8,000. The capacity of ice was found to be of the order 80, when measured by charges and discharges with a frequency of 1/10 or 1/100 second, but to have a value less than 3 when the frequency was such as one-millionth of a second. This showed that the apparently excessive capacity was to be ascribed to residual charge. Further, in the case of glass, a high temperature was found to increase the apparent capacity for comparatively slow frequency of discharge, but not for high frequency. Here, again, the difference is due to residual charge. Again, the insulation of heated glass was observed to be less after 1/50,000 second electrification than after 1/10,000 second, but to be sensibly constant for longer times of electrification. The current which flows when electromotive force is applied to a condenser is ordinarily treated as consisting of three parts, the charge proper, the polarisation or residual charge, and the conduction current due to imperfect insulation. Hopkinson pointed out the arbitrary nature of this distinction and the real continuity of the phenomena. These three terms, though separated for convenience, are really parts of one continuous magnitude. In a dielectric which exhibits residual charge and deviates from Maxwell's law, the action is essentially the same in kind as that which is found in an ordinary electrolyte.

Before noticing the large section of Hopkinson's work which relates to magnetism, mention should be made in passing of a short but suggestive paper on the Hall effect (1880), where it is suggested that the effect is completely expressed by Maxwell's "Rotatory Coefficient" of resistance, and of another "On the Seat of Electromotive Force in the Voltaic Cell" (1885), where it is pointed out that the controversy between those who held the difference of potential between metals in contact to have the value deduced from electrostatic experiments, and those who held it to be measured by the Peltier effect was one of *definition* and of hypotheses used for the expression of admitted facts.

A paper "On the Theory of Alternating Currents" (1884) discusses the action of one alternate current machine on another when the two are connected in parallel or in series. It shows that the machines will not work together in series, for they control each other's phase so as to nullify each other's effects; but that they will work together in parallel, the mutual control being then such as to produce synchronism. This conclusion was verified by experiments on a pair of alternate current dynamos intended for use at the lighthouse of Tino. Incidentally the same paper touches on the theory of induction coils, a subject which Hopkinson developed in a later paper.* There, in remarkably brief compass, a complete theory is stated of the action of the closed-circuit transformer. Later still, Hopkinson returned to the discussion of the alternate-current dynamo, and described in a paper written jointly with Mr. E. Wilson,† a series of experiments undertaken to examine the currents induced in the coils and cores of the field magnets by the movement and variation of currents in the armature.

His earliest important paper dealing with the magnetic quality of metals was published in 1885,‡ under the title "Magnetisation of Iron," and it is characteristic of the modesty of the man that in the preamble, after speaking of the work of other experimenters in the same field, he says, "I have had great doubts whether it was desirable that I should publish my own experiments at all." In point of fact, the paper is conspicuously valuable. It contains many useful data relating to samples of steel of known and very various composition, as to magnetic permeability, magnetic hysteresis, and electric resistance. One of the samples tested was the curious alloy or mixture of steel and manganese invented by Mr. Hadfield, which is almost wholly destitute of magnetic quality. The magnetic measurements were made by a novel method, each sample being a short bar, which was brought approximately to the condition of endlessness, in the magnetic sense, by being enclosed within a massive yoke of soft iron. Apart from its originality in respect of both experimental method and results the paper contains much suggestive comment on points of theory connected with the experiments. It came to be regarded, quickly and rightly, as a landmark in the development of the subject.

An examination of the magnetic properties of nickel at various temperatures followed.§ This showed that in the specimen of not very pure nickel tested the magnetic quality was lost when the temperature rose to about 310° C. But the loss was somewhat gradual over a range of some 50°, and observations of the rate of cooling from a high temperature showed no sudden liberation of heat, such

* 'Roy. Soc. Proc.,' 1887.

† 'Phil. Trans.,' A, 1895.

‡ 'Phil. Trans.'

§ 'Roy. Soc. Proc.,' 1888.

as occurs in iron as it passes from the non-magnetisable to the magnetisable state.

The behaviour of iron near this critical state was the subject of Hopkinson's next magnetic paper.* It was shown that for small magnetising forces the permeability of iron increases very rapidly as the critical temperature is approached, and then very suddenly disappears, also that the critical temperature is marked by a sudden change in the coefficient of resistance, and that it is the point at which recalescence occurs. The evolution of heat in recalescence was measured.

The same methods of inquiry were applied in the following year to certain alloys of nickel and iron, which were found to be capable of existing, throughout a wide range of temperature, in two states, one magnetisable and the other not. The state changed from non-magnetisable to magnetisable when the alloy was cooled somewhat below 0° C., but did not change back again until it was heated to nearly 600° C. In the non-magnetisable state the nickel steel was soft and ductile; in the other state it was hard. Equally marked differences were found in respect of electrical resistance. A later series of experiments deal with time-lag in the process of magnetisation, especially on the influence which the electric currents induced in the iron by magnetisation have in retarding the acquirement of magnetism.† The growth of magnetism was observed in a very massive iron core, by means of exploring coils buried at various places in its substance, and the results were applied to determine the appropriate thickness of laminated iron in transformers subjected to periodic reversals of magnetism. These experiments formed the subject of a Friday evening discourse at the Royal Institution, which concluded with a remarkable speculation as to the possibility of terrestrial magnetism being due to currents in the material of the earth sustained by its changing induction but gradually dying away.

In these and others of his later researches Hopkinson worked in co-operation with Mr. E. Wilson, his assistant at King's College, and the results were published in their joint names. The authorities of King's had invited Hopkinson, in 1890, to assume the direction of the Siemens Laboratory at King's with the title of Professor of Electrical Engineering. The post made no considerable demand on him as a teacher, but it gave him the use of a laboratory and the opportunity of suggesting to students subjects of research. He was, moreover, able to place a number of the King's College students in engineering situations, and the uniform success of the young men he favoured in this way showed that he exercised his patronage with rare judgment,

* 'Phil. Trans.,' A, 1889.

† 'Phil. Trans.,' A, 1894; 'Inst. Elect. Eng.,' 1895.

and that the students had themselves benefited greatly in coming under his influence.

Important as his various contributions to the experimental side of magnetism are, Hopkinson rendered an even greater service to the subject by his definite formulation of the theory of the magnetic circuit. This was contained in the paper on dynamo-electric machinery written in conjunction with his brother Edward,* to which allusion has already been made. The conception that the whole line-integral of the magnetic force is divisible into a series of terms for the substances of various permeability of which the circuit may be composed was as fruitful as it was simple. It threw a flood of light on phenomena which before that had received only empirical treatment. The notion of the magnetic circuit had been vaguely present to the minds of several earlier writers. Hopkinson's expression of it made it for the first time clear and convincing, and the use to which it was put in the same paper demonstrated its value on the practical side, by showing its applicability to dynamo design.

Dr. Hopkinson was elected a Fellow of the Society in 1878. He served twice on the Council, and in 1890 a Royal Medal was awarded to him for his researches in magnetism and electricity. Speaking on that occasion at the anniversary banquet, in reply to the toast of "The Medallists," he described himself as a professional man desiring to further the pure science of his subject on lines suggested by his professional work. He owed, he said, to his father his first taste for science, and to Sir William Thomson his first impulse towards research.

He married in 1873 Evelyn, daughter of Gustave Oldenburg, who survives him with three children. Three others, two daughters and a son, were killed with their father in the accident which brought Dr. Hopkinson's brilliant career to an untimely end.

A devoted lover of the mountains and an accomplished climber, he generally spent the autumn in the Alps with his family, who shared his taste and, in great measure, his skill. They spent August last at Arolla, and were much on the mountains. On the morning of August 27, Dr. Hopkinson set out, with his son Jack and his daughters Alice and Lina, to climb the Petite Dent de Veisivi, a rocky ridge above Evolena, offering no particular difficulty to a party of their experience. When they failed to return at nightfall search parties were organised, and at daybreak, on the 28th, the four bodies were found under the cliffs, roped together, having fallen from a height of some 500 feet. The cause of the accident is not known; but it is probable that the son, who was leading, slipped in consequence of a portion of the rock giving way, or that he was swept down by a falling stone. It has been well described as the saddest Alpine accident ever known.

* 'Phil. Trans.,' 1886.

The distinction and value of John Hopkinson's scientific work are so evident and so universally acknowledged, that no attempt at appraisement is required. His writings are terse enough to make careful reading imperative, but there is no trace of ambiguity or vagueness. They give an impression of easy mastery that is rare, even in work of the first class. His attack on any subject is conspicuous for its directness and severe simplicity. Any preconceived ideas which might impede it are brushed aside; nothing is taken for granted, nothing is slurred over. This indeed was a reflection of the nature of the man. Straightforwardness, simplicity, intellectual honesty were of his very essence. Admiration for his genius was not more universal than respect for his peculiarly fine character, which, moreover, compelled the warm affection of those who knew him best.

J. A. E.

INDEX TO VOL. LXIV.

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END OF THE SIXTY-FOURTH VOLUME.

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